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ORIGINAL COMMUNICATIONS.

Naval Hygiene. By G. R. B. Horner, M. D., Philadelphia, Surgeon U. S. Navy.

This branch embraces a great variety of subjects relating to the health of seamen. On it their strength, spirit and, necessarily, their efficiency depend; without it commerce must suffer, and in time of war the honor and defence of the country could not be maintained against a powerful maritime nation, whose seamen are as brave and numerous and more healthy than our own. It becomes us, then, to consider the means best adapted to prevent this, and, if possible, to render them superior in every respect. At the same time we do that, we must not neglect to refer to the means suitable to preserve the health of our extensive mercantile marine. For these purposes the first means to be taken are those intended to procure vessels properly constructed regarding the comfort and health of their occupants; for, if they be confined to small, filthy and ill ventilated apartments, other means taken may be of no use. Ships of war are often built for sailing and fighting only, merchant ships for beauty and holding large cargoes.

Hence, the quarters, in both, for the crews, and especially for the men and officers of subordinate grades, are very small proportionally to their number, besides being so constructed that pure air is nearly unknown in them. It is even common to witness this in vessels wherein the cabins and saloons, intended for the passengers and officers of the highest grades, are as spacious and airy as they can be made. In no class of vessels are we more struck with these facts than in packets and steamers. In the latter, engaged in trade between the eastern coast of the United States and the western coast of California, I observed this contrast in accommodations. While the cabin and steerage passengers were lodged mostly in separate state rooms and had sumptuously furnished saloons or cabins adjoining, the forward passengers were crowded in berths near the bows or about the forecastle. Some of them were occupied by two or three persons in each one, and were so short that no man of good height could lie in them at full length. To escape from such discomfort, which was rendered intolerable from the heat of the tropical sun added to that issuing from vast fires of coal, many passengers fled nightly to the highest decks and slept upon them. Of the sufferings of the forward passengers we may form an estimate, when informed that a number of the best accommodated cabin ones deserted their rooms and slept on the quarter deck beneath the awnings, so great was their heat. To ship builders and naval engineers belong the general construction and arrangement of vessels, and we would not recommend any medical interference with them in most matters, or where mere comfort and convenience are concerned. But in things relating to health, courtesy as well as public and private interests demand that medical men should be consulted. Whether these be or not, the former should endeavor at least to have all parts of a ship made salubrious; to have the hatches well placed for both air, light and the escape of effluvia; the air ports of large size, and so situated that they could be opened either in calm or stormy weather without the admission of rain or sea water. For the escape of heated air, animal and other exhalations, the newly adapted ventilators passing through the upper decks in parts not used as gang ways and not occupied by guns, &c., ought to be preferred. These ventilators have tops closed above and open at the sides, and answer the indications above mentioned, and from being of cast iron are very durable.

To ventilate perfectly and expel noxious gases, as well as overheated air, windsails are indispensable. They are very large and long cylinders or bags of canvas, open at bottom and expanded at top into a capacious mouth, presented to the wind, and held open by lines attached to their elongated sides. Hoops within prevent their collapse. These windsails are suspended from the rigging by other lines, and can be let down through the hatches far enough to ventilate the lowest parts of vessels. All those of our navy are thus purified, and made vastly more healthy and comfortable than ships were before the introduction of windsails.

But the part of every vessel most demanding the care of the physician is that used by the infirm, and known in men-of-war as "the sick bay." In those of our country it is almost without exception on the berth deck, either in the bows or amidships between the fore and main hatch. The former place is its location in ships of the line, and frequently the latter is that in vessels of smaller size. In our steamers the machinery will not allow of the sick being kept amidships, and abaft there is no room for them; hence from necessity they have to remain forward. This is the case even in the San Jacinto, one of our largest steam frigates, and more than 220 feet long. She indeed has no separate place for the sick; they have to lie near the fore hatch of the berth deck, and are exposed to both the heat of the galley or cooking range just abaft them, and to the intense fires in the adjacent furnaces generating steam, in the hold below. She has no gun deck nor other place of refuge (save the spar one) from such heat, for the halt or sick, by day or by night; and if it rain, or the weather be cold, or the wind strong, they have the unhappy alternative of bearing its chilling blasts, or of closing the hatches and being stewed in steam. From the description by the crew of what they suffered from these causes—the flooding of the berth deck by the sea through the horse holes at the bows of the same deck, the deluging of it by rain, the seas shipped, and the water thrown upon it in the coldest weather to clean it—we can rationally infer, that though she cannot be called a floating hell, she might be well termed a steam purgatory. Of the Saranac the same remark might be applied with good reason.

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placed on one of the gun decks, and on both or only on one side of the bowsprit. This position is most desirable from its being generally the least in the way; but when the men are letting out or heaving in a cable, or the ship is in a heavy sea, plunging deeply and buffeted by waves driven on by a head wind, it is very noisy, and often deluged by the water forced into the ports, seams and horseholes, or those through which the cables pass. The motion, too, in the bows of a vessel, is very great at such times, and renders them altogether unfit for patients ill of low fevers, from the depressing effects on the brain; or for those who suffer much from sea sickness, or who have fractured limbs. In these cases, while ships are at sea, it is always necessary to remove the patients abaft, and I usually place them near the middle of one of the gun decks. While I was in the John Adams, on a voyage from Minorca to Naples, thence to Marseilles, and back to the former port, a most striking instance occurred of the ill effects of a ship's motion on a case of the first kind, or that of low fever. The patient was a boy of that island, for the first time at sea, who suffered from excessive sea sickness and intermittent fever, to which he was liable. Accordingly, as the wind was light or strong, the sea smooth or rough, the ship in port or out of it, easy or uneasy, so the type of the fever was of ordinary height or of the lowest kind, and deprived him of nearly every sign of animation. When thus affected, his teeth were covered with dark sordes, his eyes sank, his pulse was rapid and scarcely perceptible, his face livid, and he sank into such a state of insensibility, that he was only to be kept alive by quinine, applications of turpentine and cantharides ointment, and other stimulating remedies. In this manner he lived until he got back to his native place; but when she was about returning to Naples, he could not remain in her, was sent to the Lazaretto of Port Mahon, and there died under the charge of a Spanish physician. Reference has been made to this case in my work on the Mediterranean; but it has been again introduced, as it is finely illustrative of the bad influence of a ship's motion on some patients.

To make the sick more comfortable, it is also very essential that they have a privy convenient to them, and be as little as possible exposed to being struck by the sun; that they should have well made and covered easy chairs with copper pans, which are

difficult of being broken, and require a long time to become corroded. Porcelain pans are too easily fractured, and tin ones very soon are rusted and rendered useless. A hatch directly over a sick bay is of great advantage; it affords plenty of light, admits a windsail, and enables the medical officers and patients—both those affected with disease and those injured by accident—a ready way of egress and ingress. When there is no such hatch, and a person is severely injured, it becomes necessary to drag him during sleeping hours beneath the hammocks of the crew to get him into the hospital. To do this is difficult and painful to him and carriers, from the hammocks being hung only a few feet above the deck, and obliging the latter to stoop very low while they drag the former. Another great convenience is that of having the dispensary and store room adjoining or near the sick bay, so that articles can be had for patients without delay. But in time of war the greater part of medical stores should be below water mark, and secure from damage by an enemy's shot. A single one, and especially an explosive shot, might otherwise smash all the bottles of the medical department and leave no liquids for use. This misfortune might frequently happen in our men of war of the present time, for in all of the corvettes, and in most if not all our frigates, the dispensary is on the berth deck. In former times it was in the cockpit of the last, and below water mark. Want of air and light, and the inconvenient distance of the former from the sick, have probably caused the change of place, besides many annoyances from the numerous store rooms opening into the cockpit, and its proximity to the spirit room and after magazine. When these are open no lights are allowed to be burnt, and of course the darkness prevents the compounding of medicines and other work being done. When they are immediately required, this is a great inconvenience to the sick and their attendants. In time of action, or any other, when the crew are at quarters, the wounded or injured sent to the cockpit must receive attention, and the use of covered lights is then permitted.

With a hospital arranged as above described, patients, unless over-crowded or affected with contagious diseases, will mostly improve under judicious medical advice. Should any be affected in the last manner, they ought to be forthwith sent on shore

where it be practicable, or placed in the most airy and least frequented parts of the decks, either lower or upper. There they should be hung in cots; or hammocks, if there be none of the former to be had; put behind screens, and allowed to hold no communication with any other persons than their nurses and physicians. If small-pox exist it will be very desirable that the former should have had it by inoculation or naturally, or that they be known to be well protected by vaccination. But, should this sovereign preventive have been properly practised among a crew before or after shipment, it is very improbable that they will ever become extensively infected with small-pox. Therefore, every one of a crew in the least liable to it should be vaccinated as soon as he gets on board ship, and while the vaccine matter can certainly be obtained fresh. If it be not, it will prove useless, according to my experience;—for, of the many hundred persons I have vaccinated or caused to be, in various ships of war at sea, scarcely one gave any evidence of having been infected with the virus. The same remark was made by a surgeon of much greater experience than myself. So many failures may have been owing to other causes, as gross diet—the adult state of a great majority of the crews—and their greater unsusceptibility to impression from the hardness of their skins, and loss of sensibility. Some may ascribe the failures to the influence of sea air, but none of these things are known to be facts. This, however, is an acknowledged and well proved one. Old vaccine matter has often failed to produce its peculiar effects on persons ashore with every adjuvant required to insure success. Neither the purest air, nor best diet, nor well adapted clothing and most able medical advice have been of any service. The punctures have, notwithstanding, healed and left no cicatrices, and the vaccinated have remained unprotected from variolous contagion. To escape it in very cold weather is most difficult, from it being necessary then to make vessels of comfortable temperature; from their crews being thickly clad, and seldom changing their clothes; staying much below decks; crowding together in warm places; closing ports and hatches, and stopping ventilation. When this cannot be done, they become exposed to too much cold, suffer from pulmonic complaints, rheumatism and chilblains, and hence have a choice of the two evils—contagion, or the suffering caused

by these affections. Sometimes a ship may be kept very close below and open above, as was the Brandywine in the winter of 1830, at New York. To that fault we ascribe the complication of the above diseases herein suffered. In her were mingled cases of small pox, scarlatina, typhus fever, and every other affection it seemed which could be induced by intense cold. Luckily some of the former cases were sent to the naval hospital before the crew were extensively infected. But by the time the ship left port, in March, she had lost one man from scarlatina, sent thirty altogether to the hospital, and had forty more on the sick list. In a few days after sailing, three more persons died of the same disease and typhus, and in the course of a few months others of the crew had been similarly lost. Among them was the sergeant of marines, Clargis, a noble looking, accomplished soldier, who had had a violent pneumonia terminating in mortification of the lungs. His sufferings were indescribable; so great and protracted were the dyspnœa and difficulty in expectorating the exceedingly fœtid sputa which for day after day were discharged, to the disgust of the whole crew.

So much was due to the fitting out of a man of war in winter! a practice ever to be condemned or avoided. There are two other practices in ships almost as condemnable; one is, that of dry *holy stoning*, or rubbing the decks with huge stones and sand; the other practice is that of deluging the former with water in the coldest weather, even when it freezes as soon as thrown upon them. Dry holy stoning is intended solely for good appearance and cleanliness, washing decks for the same purposes and to keep them tight. In doing this the men generally go barefooted, and suffer proportionally from the conjoined ill effects of cold and moisture, inducing rheumatic and pulmonic complaints. By holy stoning the chief mischief is done to the eyes and lungs, which inhale the impalpable sand floating in the air, and become irritated. To this cause is attributed the death of the master-at-arms belonging to the vessel named, and who became affected with phthisis pulmonalis. Cases of ophthalmia arise and are aggravated from the same cause. But when the sand is wet it may be rubbed on the deck without injury; and in mild weather washing them does little harm—unless on the lower ones—from their continuing wet a long time, and may be

of advantage, from the men getting their feet cleansed, who, if the decks were not washed, would seldom have them so. To obtain a like condition of the whole person, bathing in fresh or sea water should be practised by a crew, whenever it be of moderate temperature. Though buckets and tubs sufficient could not be had for a large number of men, they might bathe on a beach, or at sea, by having boats rowing about them and letting down sails into the water by the four corners for the use of inexpert swimmers, as I have often witnessed, when the sea has been calm. Bathing, besides being a very healthy practice, from its tonic and cleansing effects, is a fine amusement, and for this reason, if no other, ought to be encouraged. Like music, dancing, playing at dice, chess, draughts and dominoes, it takes away the monotony of sea life, makes time pass pleasantly, and creates good will in a crew. Water is still more highly recommended for drinking, and unless it be pure, it is vain to expect good health in any vessel on a voyage, and cut off from the means of procuring substitutes when it is impure. When good, it is the only drink seamen can use freely with general impunity ; and nothing laid in for their sustenance makes them so efficient and so comfortable. It is an indispensable article with them, and one for which no substitute can be found. This is not so with any other ; if the pork be bad, the beef may be good; should the bread be mouldy and wormy, the rice may be sound ; if the cheese be spoiled, an additional quantity of butter can be allowed. The same may be done with any other part of the rations. Bad water, too, may be as injurious in port, and sometimes more so than at sea, when sailors at the same time indulge in eating fresh meat, fruits and vegetables, especially the crude, unripe and acid ones. If these be eaten at the same time that brackish water is drank, or that obtained from fountains and rivulets just after heavy rains, and charged with earthy matter, they never fail to produce bowel affections. The greatest number which have ever come under my notice have been caused in this manner ; and I always expect as a matter of course, when between the tropics, or it be summer anywhere, to have at once a large number of such affections to treat. But by far the largest number have been seen among crews drinking the water of the La Plata, or that taken from its shores. Neither should it be used before

it is purified by being allowed to work, or by strainage, or throwing into each cask a small amount of powdered alum, which is thought at Buenos Ayres the best purifier, and ought to be proportioned to the quantity and impurity of the water. For the consumption of a small number of persons, dripstones may furnish as much pure water as they may need for drink; and in the Mediterranean, from the vast quantity of porous sandy limestone found on its shores and islands, these stones can be easily obtained at a low price. At Malta they are very plentiful, and so cheap, that every vessel touching there ought to lay in a supply for both present and future use; for after they have been used for some time, the impurities of the water strained fill their pores and impede its flow, so much as to prevent this being abundant. Should a crew be large, other means must be adopted, and none answer so well as large casks, set on end like a common scuttle-bat, and having either a strainer at bottom made of several layers of bunting or of hair cloth, resting on a grate and covered with gravel and sand; or having a patent strainer of silver gauze, with a clean sponge fixed inside of it. The frigate Savannah had a strainer of the latter kind, which was attached to the scuttle-bat when necessary, and was thought very good. For the use of my patients in the Delaware I employed a strainer of the former kind, holding only five or ten gallons of water, and for the same purpose in the Savannah used a large stone filter, which had a double bottom and a chamber above, with a hole in the centre into which a sponge was placed. The water ran through this, thence percolated through a layer of sand and a strainer beneath—got into the lowest chamber and was drawn off by a stop-cock in a limpid, pure condition. For a sick bay I prefer this filter* to any other, from its convenient size, easiness of being cleansed and repaired, and so well answering the purpose for which it is designed. Other filters perhaps may be obtained to answer as well, and there is no excuse for a vessel leaving, at least any of our ports, without having some of them. In steamers there is less demand for them, from the great ease with which their immense fires can by means of retorts or alem-

*It is said to be one of Wainwright's of Staffordshire, England, which have gravel and charcoal mixed with the sand, contained in the middle or closed chamber.

bics, convert salt water into fresh. Whether water thus distilled is as wholesome as that impregnated with some saline or earthy matter, is a subject of doubt, as in some persons it has caused diarrhœa, and it, moreover, when so purified, is insipid and unpalatable.

Of the best methods of preserving and purifying water, much has been written. Some have recommended a small amount of quick lime and sulphuric acid to be put into each cask; some have used muriatic acid, others calcined lime or pure charcoal, with a little sulphuric acid; others have put in powdered charcoal alone, or charred the casks inside. This is a very common method of purifying them when old, and though it may not have any great efficacy in preserving the water, it certainly has some in preventing the decay of the casks. The charcoal itself, likewise, when put into the water, acts both as an antiseptic and destroyer of fœtor arising from the formation of sulphuretted hydrogen. But I prefer filtrating impure water through charcoal, when confined in a chamber or other place suited to hold it and prevent its rising to the surface, as its lightness will certainly cause it to do when thrown loose into the water.

Provisions.—Such a variety of them are now used at sea, that it would be superfluous to enumerate them or do more than write a few remarks respecting some of the principal. Of these the two most used are bread and meat. The former in all ships is commonly biscuit made of wheat flour,—sometimes of this and rice mixed, when the former is very dear, as in Brazil. The hardest bread I ever saw was obtained at Bahia, and was said to have been thus made. It was too hard for any ordinary teeth to chew, and required hammering to be got into fragments. It is hardly necessary to say that such bread, and the wormy, mouldy and sour, should be condemned. Indeed biscuit generally is difficult of mastication and digestion, from its toughness, and is neither savory nor wholesome, unless dried and crisped in an oven just before it is eaten. Next to bread, rice may be ranked as the most valuable vegetable food. Though it is insipid, and constipates most persons, it can be easily rendered palatable by seasoning, and prevented from producing costiveness by mingling it with stewed fruit or molasses. For the sick

I have found rice invaluable in bowel affections, either in the form of a gruel or drink, or when simply boiled and flavored with any pleasant condiment. In this state my patients, almost without exception, have preferred it to arrow root or any other of the farinaceæ, which have a flatness of taste—evils to be overcome only by high seasoning with wine, sugar, lemon juice and spices, rendering them as rich as prohibited meats. Of these, any which are preserved by being first cooked—by boiling or roasting—are to be preferred for the sick or well, to all dried ones, or any preserved by being put into brine, either of salt alone or that and nitre. Both of these articles so impregnate flesh of all kinds as to make it hard and insoluble in the gastric juice. It then creates irritation of the stomach, causes unnatural thirst, and is altogether difficult of assimilation, besides impregnating the system with a superabundance of saline matter. The first ill effects observed afterwards are ulcers in the mouth, phlegmous in different parts of the body and limbs, and finally scorbutic symptoms. To avoid, then, these and other evils resulting from salt provisions, as little of them as possible ought to be used, and when they are, they should not be cooked before they have been first soaked at least twenty-four hours in fresh water, and subsequently washed in it after it has been changed. Preserved fresh meats are dear in appearance, but if we calculate their superior niceness and wholesomeness; their freedom from bone, gristle and other useless parts; their not requiring more fire than enough to rewarm them, and the very little space occupied by the canisters containing them; the very perfect manner in which these meats are kept sound for an indefinite time, and the vast amount of salt meats lost from corrosion by the brine, and putrefaction from various causes, we may very safely conclude that the latter meats are dearer than the fresh preserved. On the coast of Brazil, where these cost 45 cts. per pound, I found them decidedly cheaper than live fowls at six dollars per dozen, from the great mortality among them at sea. Even when they do not die, they often become sickly, waste away, and afford poor nourishment. The same may be said of other live stock, save, perhaps, pigs; and when not correctly, it is in rare instances,—from fine weather and great care in feeding. When this is not done, they are illy watered—the vessel labors much, tosses

them about and keeps them wet; they soon droop, get feverish, and become unfit for use by the sick or well.

With fresh preserved meats we must likewise recommend vegetables which have been cooked in a similar manner and hermetically sealed in tin cases and bottles. There are also a large quantity of vegetables now reduced by heat to a very dry state and sealed in tin boxes, but when taken out and put into boiling water, resume their wonted freshness and plumpness. The best I have seen were prepared in France, and had been sent to the Chief of the Bureau of Provisions and Clothing of the Navy, for inspection and trial. They gave such satisfaction that we hope they will come into general use and be prepared on a large scale in our own country. In addition to the above articles, preserved soups, custards and milk ought to be laid up for sea stores, at least for short voyages, and as is now practised in some packets. By the aid of ice it may be easily effected, though the articles be not put in sealed vessels. In ships sailing from countries where ice abounds, it might be stored away at a very low price, in sufficient quantities to last for months, without any great inconvenience as regards room. On my passage in the Crescent City from Chagres, all our meats were said to have been preserved on ice laid in at New York, abundantly enough to last both during her outward and homeward passage. Of spirits, wines and malt liquors, it is necessary to procure a supply for culinary and medicinal purposes. Habit, too, renders them requisite for some persons, and the palates of others may make them indispensable as articles of luxury. But for the treatment of invalids, I have by long experience found them of minor importance with respect to articles decidedly medicinal. As diffusible stimulants in cases of emergency, they are serviceable; but as tonics or medicines, used to produce a permanently invigorating effect, they are much inferior to the vegetable bitters, and if any stimulation be required, it is much better to infuse these in a small amount of spirits mingled with water. This also is preferable, because it prevents patients from having it in their power to say that the "doctor recommended them to drink spirits," and to urge this as a plea for their continuance when well.

The next subject claiming our notice, and the most important to the sick, is the laying up of all the medicines, implements

and appliances needed for them. In their selection we must determine how many are needed, and next the quantity of each one which may probably be required. In determining this we must be governed by the space allowed to accommodate them, the number of the crew, the length of the proposed voyage, and the amount of sickness anticipated. The ability or inability to procure a re-supply where the vessel is bound, and the abundance, cheapness or dearness of certain medicines there, ought also to regulate the quantity to be primarily obtained. It is very bad economy to fail getting a full supply of articles which are exorbitantly high abroad, and equally bad to lay in such at home when they are perhaps native products of the countries to be visited and correspondingly cheap in them. This is a familiar blunder, and committed in many vessels; and the only good reason for it is that it encourages domestic commerce and causes money to be expended at home. While laying in medical stores also, we ought as much as possible to obtain the most efficient of every class—to get them in the most concentrated form, so as to occupy the least space—to put aside crude articles and procure their active principles—and to have all medical stores, whether in drawers or boxes, well secured against rats, mice and vermin, by means of tin or other metals, and to have these well painted externally, to prevent corrosion by rust. The medicines likewise should be put in packages and bottles, perfectly air and water proof, and adapted in shape to the places wherein they are to be kept. As a general rule, the former store most conveniently and take up least space when square. It will be found in fitting up either a medicine chest or a dispensary, that when round they require about one-half more room—a vast deal to lose when not enough is allowed, although storage is made in the most economical method. One of the strongest objections to laying up an overplus of medical stores, or not packing them closely in a dispensary or chest, is that it makes it necessary to put the surplus in the hold. When this happens, they are apt to be lost and pillaged; they cannot be got for immediate use without inconvenience; and give so much trouble to those having charge of the hold that they complain, and quarrels result between them and the persons belonging to the medical department. From medicines being kept in the hold I have frequently,

under the belief that there were none of a certain kind in it, obtained a fresh supply, when an abundance was there, as proved by their having been subsequently found. On one occasion I was positively informed that no alcohol and Epsom salts were in store; a re-supply was ordered and purchased, and after it had been brought on board, four gallons of the first article and a barrel of the other were discovered in that part of the hold called the spirit room. The prohibition of lights in it makes such oversights more easy; but in the latter instance it was attributed to the inscription on the head of the barrel having been covered with whitewash.

In procuring medical stores, we should likewise take into consideration the climate wherein a vessel will have to cruise or remain longest. Accordingly, if the climate is to be a cold one, we ought to lay in a full supply of all articles adapted to the treatment of rheumatism, pneumonia and other affections caused by cold. If the climate is to be a hot one, we should get an extra supply of mercurials, opium, saline compounds, acids and other remedies suited for miasmatic fevers, dysentery, diarrhœa, cholera and affections incidental to warm latitudes. In temperate climates, when we are to have an alternation of heat and cold, as in our own, we must of course obtain a supply of medical stores adapted to both. The same ought to be done in regard to clothing, and seamen of every class should be provided with such as may enable them to be well protected in any change of weather. From a want of information concerning the dampness and chilliness of that on the coast of California, it must have happened that a large amount of summer clothing was sent to our squadron there, instead of winter clothes, and occasioned great discomfort to the seamen during my stay on that station. For hot weather linen is most comfortable, but cotton is preferable, from its medium quality between the former and woolen goods, as well as for the much greater quantity to be bought for the same money. Cotton, also, does not, like linen, allow perspiration to be suddenly checked, nor does cotton so absorb perspiration and remain sodden with water. Hence muslin sheets are infinitely superior to linen for sea use. For overcoats, in frigid regions, woollen cloth is indispensable, but in rain, gum-elastic and oiled are preferable, from their lightness

and superior impermeability. In sultry latitudes, the last named is to be preferred to the second, from its being both waterproof and not adhering together so closely when one part touches another, that it becomes inseparable or cannot be drawn asunder without being torn and spoiled. A vast amount of gum elastic cloth is thus ruined. From its neatness and flexibility, however, it is an excellent article for mattress covers, counterpanes, capes, coats, cloaks and pantaloons in damp and mild climates, and may be very useful in the most sultry, when care is taken to keep apart the different folds and to prevent any portion of the side on which the gum is spread from touching any other, when the articles are laid by and not in use. Considering how much these waterproof clothes contribute to comfort and health, it is wonderful that they have not yet come into general use for sailors, and that none are provided by Government for them. They make much less use of gum-elastic shoes, which are so much worn on land, and so indispensable in keeping the feet dry. When gum-elastic overshoes cannot be got for this purpose, an excellent substitute will be found by wiping off all dirt and blacking from shoes or boots, and then several times smearing them with a composition of gum-elastic clipped to pieces, softened with pure spirits of turpentine and dissolved by boiling in whale oil or some other animal one. For the soles, several coats of copal varnish will answer the same purpose; and this is to be preferred, from its giving them greater firmness and durability.

Besides the ordinary articles of the materia medica to cure disease, we have to get a supply of others to prevent it, or those termed disinfecters. The most commonly used, and one of the most efficacious is chloride of lime, but it is objectionable from its great bulk and weight proportioned to the amount of chlorine it contains, and from its deliquescing when exposed to moisture. The other chlorides, as those of zinc and lead, are therefore preferable; but for instantaneous disengagement of chlorine I prefer the mixture of table salt, sulphuric acid and black oxide of manganese in due proportion—that is, having three parts of this to four of the first article and four of the second one, diluted in four parts of water. Sulphuric acid, also, poured on nitre, disengages a large amount of disinfecting gas or nitrous acid fumes, and constitutes the famous one of Dr. James Carmichael Smith,

of Dublin. The proportion is a half fluid ounce of the acid to two drachms of the salt. The burning of sulphur and nitre together, the fumes of gunpowder, tar, tobacco and vinegar, and white washing with slacked lime or chloride of lime, may likewise be useful; and we may employ Labarraque's solution of chlorinated soda, which is certainly good to destroy the fœtor of sores and wounds. We also may use Sir William Burnett's solution of the chloride of zinc, and yet not have a healthy ship if she be not cleaned, particularly in the hold, where such accumulations of noxious substances collect. No disinfecting agent could be got to counteract their deleterious action on a crew, and all the patent air pumps which could be worked by them could never throw out of a ship thus loaded with filth all the poisonous gases generated in her during a long sojourn in a hot climate. Nor when small pox or any other truly contagious disease is prevalent on board, can we trust exclusively to any such means for disinfecting the air in a vessel, and rendering it innocuous, as well as inoffensive to our senses.

Experimental Researches applied to Physiology and Pathology.

By E. Brown-Séquard, M. D., of Paris.

(Concluded.)

XXXI.—THE AUDITIVE NERVE IS A NERVOUS CENTRE.

In an anatomical point of view there is no doubt that the auditive nerve is a nervous centre. This is proved by the fact that cells of gray matter are found, not only in the terminal part of the nerve, but also in its trunk, in many animals, according to the researches of Stannius, Corti, Kölliker, and myself.

In a physiological point of view, the fact I have discovered, (see Art. V. p. 21,) viz., that any injury to the acoustic nerve produces turning, is sufficient to prove that it is a nervous centre.

The degree of pain produced by an excitation of this nervous centre appears to be as considerable as that caused by a similar excitation of the trigeminal nerve. I will publish soon an account of the strange effects produced in different parts of the body in consequence of an injury of that nervous centre. I will merely say here that, after such an injury, there are muscles which

appear to be slightly paralyzed. Besides, there seems to be a notable hyperæsthesia of the skin everywhere.

Flourens has found that a section of the semi-circular canals in birds and some mammals produces a peculiar disorder in the movements of the head, and, in some cases, turning. He says that the auditive nerve must be considered as composed of two nerves: one going to the semi-circular canals and possessing a peculiar power on the movements of the body, and the other, the vestibular or true auditory nerve. What I have found on frogs is in opposition to these views. A section of the semi-circular canals, in these amphibia, does not produce any effect on the movements of the body, and the slightest excitation of the true auditive nerve is sufficient to produce pain, hyperæsthesia, turning, and other strange effects on many muscles of the body.

I have sometimes seen turning produced after the mere laying bare of the kind of bladder, containing the terminal part of the auditive nerve, in frogs. So slight may be the excitations on that nerve sufficient to produce turning, that very likely turning after the laying bare of that bladder was the result of some slight mechanical injury of the nerve. The rapidity of turning and the smallness of the circle then described are in proportion to the degree of injury to the nerve. When the two auditive nerves are injured, the animal turns on the side most injured. Sometimes, instead of turning, the animals roll around the longitudinal axis of their body; this takes place in very strong animals after the terminal part of the nerve has been entirely crushed.

In frogs deprived of their cerebral lobes, the same effects are produced after injuries of the auditive nerve, as in unmutilated frogs.

XXXII.—ON APPARENTLY SPONTANEOUS ACTIONS OF THE CONTRACTILE TISSUES OF THE ANIMAL BODY.

All the contractile tissues of the animal body (the muscles of the trunk and limbs, the muscular layers of the digestive canal, the iris, the uterus, the dartos, the cellular tissue, etc.) present, sometimes, apparently spontaneous contractions. I give this name to contractions which are not the result of an external

excitation or of an excitation produced by the nervous system on the contractile tissues. These contractions may be permanent or momentary, rhythmical or irregular, slight or very powerful. One of their causes, if not their only cause, appears to be an excitation directly produced on the contractile fibres by the carbonic acid existing in the blood.

1. *Contractions in the muscles of the face after a section of the facial nerve.*—My friend Dr. Martin-Magron and myself have discovered that after the section of one of the facial nerves, on a rabbit, the face becomes very quickly deviated, not on the healthy side, as it is known to be in man, but, strange to say, on the paralysed side. The deviation, very slight at first, increases gradually during one or two weeks, and then it is so considerable that the middle of the lips is at a distance of four, five or six lines from its natural situation. There is an evident state of contraction in all the paralysed muscles. When the animal is excited, or when its respiration is somewhat disturbed or prevented, the paralytic muscles tremble, and sometimes they have rhythmical contractions and relaxations.

The contractions of these muscles may be so considerable that the bones themselves, and, secondarily, the teeth, may be deformed. In one case, on a rabbit which I had kept living twenty-one months after the extirpation of one of the facial nerves, not only the superior and inferior jaws were by far less developed on the paralysed side than on the other, but the anterior part of the superior maxillary bone was deviated towards the paralysed side, so that the middle line of the roof of the mouth was curved and presented a great concavity on the paralysed side and a corresponding convexity on the other.

When the two facial nerves have been divided, there is no deviation, but there is an evident state of contraction in all the paralysed muscles, particularly around the lips.*

When one of the facial nerves is divided on a dog, on a cat, or on a guinea pig, there is generally no deviation on either side. But very frequently there are convulsive movements, and sometimes rhythmical contractions, in the para-

* Dr. Martin-Magron and myself have found that death occurs from inanition in all the species of mammals on which we have divided the two facial nerves. After the operation they cannot swallow: we do not know why.

lyzed side of the face. One of these two kinds of movements always exists in young cats. They are increased, or produced when they do not exist, in dogs and guinea-pigs, almost every time we prevent the animal from breathing freely. Once, on a very vigorous guinea pig, upon which one of the facial nerves had been torn away, I saw alternate contractions and relaxations taking place, without a relapse, for eight or ten days after the operation in the paralysed muscles. After that time, these tremblings appeared only when the circulation and the respiration were rendered very active, or when the respiration was prevented or diminished. In the case of an impaired respiration, the strength and frequency of these movements were in proportion to the degree of asphyxia. During many months, the same phenomena existed in this animal.

I ought to say that in all the experiments above related, the nerve could not have any share in the movements, because, the fifth day after the division, or after the extirpation of a portion of it, the peripheric part had entirely lost its vital property.

In man, as Dugès justly remarks, as long as there is no attempt at movement, voluntary or emotional, the face remains without any deviation, in cases of facial hemiplegia, which have not lasted a long time.

2. *On spontaneous rhythmical or irregular contractions in muscles of animal life, after death.*—It is a very important fact in connection with the theory of the action of the heart, as I will try to prove hereafter, that other muscles, and particularly muscles of animal life, are capable of having rhythmical movements. This fact I have discovered in the following cases:

a. After the division of the nerves of the ischiatic and lumbar plexuses, on one side, in mammals, if we suddenly asphyxiate the animal, we see, at first, convulsive movements in the three limbs and in other parts of the body not paralysed. After one, two or three minutes, these movements cease, and there are only some tremblings in the muscles of these parts. The paralysed limb has no movement at all during one or two minutes, after which time, suddenly, contractions in many bundles of muscular fibres partially take place. In the same bundle the contractions sometimes appear to come regularly one after the other. In

some cases I have seen, besides these tremblings, movements of the entire limb, consisting of some successive flexions and extensions of the limb, and after these movements had ceased, contractions limited to various bundles of fibres appeared. In these cases the action of the muscles began very late after death, and once, only six minutes after the beginning of asphyxia, which lasted two minutes and a half.

b. Nearly the same movements of which I have spoken as existing frequently in the face, during life, in rabbits and guinea-pigs, after the section of the facial nerves, exist always, either during agony or a little after death. They are generally produced by partial contractions and relaxations of the different bundles of fibres of the various muscles. It is rare to see all the bundles composing one muscle contracting together. These phenomena last five, six or eight minutes after the last respiration. There are also such movements in the face during agony and after death, when the nerves have not been cut and when there is no paralysis; but then the movements appear later and do not last so long as in paralysed muscles.

c. I have seen in many rabbits apparently spontaneous rhythmical contractions in the respiratory muscles. In about ten rabbits, out of forty or fifty, the following phenomena were very decided; on the others they were slight, and sometimes very slight, but in all cases a part of them always existed. I open the abdominal cavity and expose the bowels to the action of a cold atmosphere, so as to lower the temperature of the animal; after some minutes I make a little opening in one side of the chest, and, at last, after a few minutes more, I open largely one side of the chest. Generally, in such circumstances, the respiratory movements continue to take place with energy. I then take away the sternum and divide the two diaphragmatic nerves. The movement of the diaphragm, nevertheless, continues, and it exists rhythmically together with the movements of the other respiratory muscles. Six, eight or ten minutes afterwards the movements of the diaphragm are still regular, (there are from five to twenty contractions in a minute;) the intercostal muscles present then only partial contractions. The different bundles of fibres of these muscles contract separately one after the other, but the same bundle has generally regular contractions and re-

laxations. At that time I destroy the spinal cord, and see that the movements of the diaphragm and of the intercostal muscles are not changed after this operation; they last for nearly a quarter of an hour, and in some cases much longer; their regularity subsists. In the diaphragm, long after the general movement has stopped, there are regular or irregular contractions of many bundles of fibres for one, two, three hours, and sometimes more.

3. *Deviation of limbs produced by a contraction of paralysed muscles.*—In pigeons, after the destruction of all the lumbar part of the spinal cord, the two posterior limbs are completely paralysed. The muscles then are soft, and the different parts of the limbs do not resist at all, when we try to put them in flexion or in extension. But after a few days the paralysed muscles become harder, and after a few weeks there is an evident state of contraction in them. The limb is generally kept in a state of extension, and deviated on one side or the other. The deviation becomes considerable after some months.

Very likely it is owing to the same cause that club-foot and other deviations are produced in embryos, after a destruction or an absence of development of the spinal cord.

4. *Rhythmical movements in the eye of the Ink-fish.* (Loligo sepia, L.)—The ciliary muscle so well described by Dr. W. Clay Wallace, of New York, in the eyes of superior animals, is strongly developed in the ink-fish. After an eye of this mollusc has been separated from the body, I have sometimes found very singular and perfectly rhythmical movements produced by the ciliary muscle. These movements consisted in alternative contractions and relaxations of some parts of that muscle. At every contraction a notable depression was produced in one portion of a zone corresponding to the circumference of the cornea.* In one case I have found four times in fifteen minutes the same rhythm existing in one part of the ciliary muscle. At each of these four examinations I have found sixteen contractions in one minute.

5. *Spontaneous Contractions of the Uterus.*—I have seen hundreds of times the uterus or its cornua, full or empty, contracting to appearance spontaneously, after the death of rabbits and other animals, at a time when the spinal cord had entirely lost,

* The eyes had not been opened.

not only its reflex power, but also the power of acting on muscles when directly excited by galvanism, by warmth or mechanically.*

I have also seen movements taking place in the uterus and in its cornua, in recently dead animals, the spinal cord of which I had destroyed in all its length. The same movements I have found after I had taken out from the abdomen of a living animal the whole uterine apparatus. I have found sometimes that after I had put a ligature around the trachea of guinea pigs, which were at the end of gestation, parturition took place and was produced by three causes: 1st, a direct excitation of the spinal cord by the venous blood; 2d, a direct excitation of the uterus by that blood; 3d, a reflex action of the spinal cord. In two cases I have seen delivery taking place after the action of one only of these three cases, namely, the direct influence of black blood on the uterus of the Guinea pigs, the spinal cord of which I had destroyed from the sixth costal vertebra to the sacrum. The more complete and sudden is the asphyxia, in a rabbit or a Guinea pig, during labor, the more certain will the delivery take place.

Dr. Tyler Smith speaks of a peristaltic action of the uterus, which may expel the child when the mother has died during labor, undelivered. He has not attempted at all to explain that con-

*Dr. Tyler Smith, in his very original book on Parturition, (London, 1849, p. 40,) says that "a slow reflex action of the uterus may possibly continue long after the rhythmic respiratory actions have ceased; as long, indeed, as the body retains its warmth." There is a great error in these lines, about the relation between the warmth of the body and reflex action. We may observe reflex actions even in animals that have lost 10, 12 or 15° Cents., (18, 22 or 27° Fahr.,) of their temperature, and, in certain circumstances, these actions may be, then, more powerful than if the temperature of the body was normal. For instance, if we decapitate an animal after having put a ligature around the carotid and vertebral arteries, we find, when pulmonary insufflation is made carefully, that two important phenomena take place—one is a gradual rapid loss of temperature, if the atmosphere is cold, (this is the well known fact discovered by Sir B. Brodie,) and the other is a gradual and considerable increase of the reflex faculty. It has been in such cases that I have found the greatest degrees of reflex power in mammals. The nervous power accumulates to such an extent in the spinal cord, that if we pinch the skin in any part of the body, but more particularly on the chest and on the anterior limbs, a reflex respiratory movement takes place.

traction, *i. e.* to find out its cause and the circumstances which favor or are opposed to its existence; besides, he has not demonstrated that the peristaltic contraction is entirely independent of the nervous system.

6. *Spontaneous rhythmical movements in the crop and œsophagus of pigeons and other birds.*—I have found that if the crop of a bird, and more particularly of a pigeon, is opened during digestion, some rhythmical movements are frequently seen in it and in the œsophagus. Ordinarily these movements are perfectly regular. They begin in the upper part of the crop, and are propagated from there to the œsophagus. If the animal is asphyxiated, these contractions become very energetic. Their ordinary number, in a minute, varies from ten to twenty.

I have ascertained that these rhythmical movements take place as well in a crop and œsophagus separated from the animal, as in these same parts left *in situ*. Therefore, the nervous centres are not the source from which originates the excitation which acts on the muscular fibres to put them in contraction.

7. *Spontaneous movements in limbs of persons who have died of cholera.*—It is known that after death by cholera, the whole body, and more particularly the limbs, have sometimes very considerable movements. In some cases I have seen alternative movements of flexion and extension of the arms or of the legs, even three hours after the cessation of the beatings of the heart. Physicians who know how quickly after death the nervous system loses its vital powers, will admit easily that these movements cannot be the result of an action of that system. I have ascertained on more than sixty bodies of men who died of cholera, or of various other diseases, that a short time before, or a very short time after the cessation of the beatings of the heart, no reflex action was produced by the tickling of the sole of the foot. The greatest duration of reflex action that I have observed after death has been in a case of cerebral apoplexy. It has lasted thirteen minutes after the last breathing, and about eight minutes after the last beating of the heart. Dr. Bennet Dowler has recorded many curious facts (observed in cases of death from yellow-fever, cholera, etc.,) from which he concludes also that the movements taking place in the limbs are not reflex actions.

I have found that, in general, the more sudden and complete

has been the asphyxia before death, by cholera, the more the limbs are moved after death. I have found also that it is in patients who have died during the algid period that these movements are ordinarily found.

These facts, as I will show hereafter, appear to prove that these movements, like the other movements, of which I have previously spoken, are excited by carbonic acid alone, or together with the poison of cholera.

8. *Spontaneous contractions of the bowels, the bladder, the iris and other parts of the body.*—It is known that frequently at the time of death, many of the contractile tissues of the body are put into contraction. I can go farther and say that it is so with *all* the contractile tissues; and that, contrary to the general opinion, a nervous action is not necessary for these contractions.

There are contractions in all the following organs or tissues during agony and after death: 1, the muscles of animal life; 2, the sphincter of the anus; 3, the respiratory muscles; 4, the iris; 5, the digestive canal (in all its length); 6, the urinary bladder; 7, the uterus; 8, the scrotum, (dartos); 9, the gall-bladder; 10, the ureters; 11, the seminal vesicles; 12, the bronchial tubes; 13, the skin; 14, the blood-vessels; 15, the lymphatics; 16, the cilia.

As to the skin, in many cases the so-called *goose-flesh* (cutis anserina) takes place a little before or little after death, although the body has not yet become cold. I have seen it very strongly marked on the inferior limbs of a paraplegic who died of a softening of the dorso-lumbar part of the spinal cord. It results from this fact that the cellular tissue is able to contract from the same cause which produces contractions at the time of death, in muscular tissues—that is, very likely, carbonic acid.*

* Kolliker has recently discovered fibro-muscular cells—that is, muscular fibres of organic life—in the skin, and he maintains that the *cutis anserina* is produced by these fibres, and not by the cellular fibres. I have published facts which, I think, prove conclusively that the contractions in the skin are in a great measure performed by the cellular tissue. (See Comptes Rendus de la Soc. de Biologie, 1849, t. i. pp. 134 et 157, et 1850, t. ii. p. 132.) Since that time, I have found that in some cartilaginous fishes, in which the iris does not contain any muscular fibre, and is composed of cellular tissue, this membrane may be the seat of considerable contractions; so that I consider it as perfectly certain that the cellular tissue (at least in some organs) is contractile.

Besides, I have found contractions of the cellular tissue of the skin of the face, in animals killed by asphyxia, and on which the facial nerve had been divided for many days or weeks.

In the same man who had a paraplegia, and of whom I have just spoken, I saw very strong contractions in the dartos, during agony.

In animals suddenly asphyxiated, after the destruction of the dorso-lumbar part of the spinal cord, the seminal vesicles sometimes contract, and a slow ejaculation takes place, although there is no erection.

In the sphincter of the anus, when it is paralysed, there are only slight contractions, but they are evident.

The urinary bladder, during agony or after death, sometimes contracts so much, even when it is paralysed, that all the urine it contains is expelled.

The ureters present very strong contractions, in animals recently killed by asphyxia, and these contractions in some cases are rhythmical. The same movements are seen when all the urinary apparatus is *in situ*, and when it has been removed from the abdomen, and therefore separated from the nervous centres. The contraction begins at the kidney and thence is very quickly propagated all along the ureters to their termination in the bladder. Among the contractile tissues, that of the ureters is one of the most irritable.

Bidder and Schmidt, of Dorpat, have recently found that after the division of the two pneumogastric nerves, there is more carbonic acid expelled by the lungs than usual. This fact is very important, because if the theory, which I am about to propose, be true, we ought to see a contraction produced in the bronchial tubes, in consequence of the unusual amount of carbonic acid that they contain. Now, such a contraction certainly exists then, and it is it which causes the well-known difficulty in the expansion of the chest, which exists in that case.

In the eyes, even when they are paralysed by the section of the three nerves of the iris, (the third pair, the sympathetic, in the neck, and the ophthalmic nerve,) the pupil may, at first, contract and afterwards dilate very much.

The lymphatics and the thoracic duct contract very much after death. I have, sometimes, in cases where these vessels were

dilated by chyle, introduced a glass tube, two lines in diameter, into the thoracic duct, and I have seen the liquid ascend into the tube, and in one case run out, although the tube was five inches high.

The cilia are known to have movements independent of the nervous system.

The gall-bladder contracts little and slowly, but evidently, after death, even when it has been, with the liver, removed from the abdomen and separated from the nervous centres.

The choledoch duct and the pancreatic duct, as my friend Cl. Bernard has discovered, have rhythmical contractions during life, in birds. I have found these movements perfectly regular after I had removed all the viscera from the abdomen. Therefore the cause of these rhythmical contractions is not in the nervous centres.

The bowels have considerable contractions during agony and after death; and I will prove hereafter that the cause of these movements is not the influence of cold, or that of air, when they are exposed to the atmosphere. Nurses, in France, are in the habit of judging that death has positively taken place, when, after the cessation of breathing, they see urine and fæcal matters expelled. This expulsion depends upon the contractions then taking place in the bladder and in the bowels.

9. *Causes of the apparently spontaneous contractions during life and after death.*—All the contractions of which I have spoken, appear to me to be produced by an excitation made upon the contractile tissues by a substance existing in the blood, and the quantity of which becomes much increased during asphyxia. The relations between these contractions and asphyxia are evident. A great many of them do not exist unless asphyxia exists, and their energy is always in proportion to the degree of asphyxia.

I believe that the substance in the blood which has that power is the carbonic acid. In admitting this opinion we can easily explain all the phenomena.

There are certain contractions which take place in muscles of animal life, after death, and which have quite another cause. In the cold seasons, it is not uncommon to find, in limbs of frogs, when we separate them from the body, apparently spontaneous

contractions, lasting sometimes for half an hour or even more; but these contractions have begun when we have cut the nerves, and they continue on account of galvanic discharges which accompany them. The fact that they begin after the excitation of a nerve, is sufficient to show that they are not like the other contractions, of which I have previously spoken.

Some of the facts I have related may appear to be distinct from the others. So, for instance, contraction taking place in paralysed muscles of the face or of the limbs in living animals, might be considered as quite different from the contractions existing after death. I think that they originate from the same cause, viz., an excitation by carbonic acid. A muscle may be moved or not be moved by an excitant. If the degree of irritability is greater in one case than in another, we may see the same amount of excitation produce a movement in the first case, and not in the second. If the amount of excitation increases, then we may see both muscles moved, but the most irritable more than the other. This is sufficient to explain why the paralysed muscles may be moved by the carbonic acid existing in the blood during life, while the muscles that are not paralysed are not moved. I have found that the degree of irritability increases, during a certain time after paralysis, in the muscles of animal life. Their irritability being augmented, they are excited sufficiently to contract, by a quantity of carbonic acid which is not sufficient to act on the other muscles.

The following facts and reasonings will, I believe, prove that, at least in the bowels, black blood, very likely by its carbonic acid, may excite powerful movements. It is known that when we open the abdomen of an animal immediately, or a short time, after death, we generally see considerable movements in the bowels. These movements have been attributed to the action of air, or to that of cold, on the bowels. This is not a right view. A sudden exposure to a cold atmosphere may, possibly, produce contractions in the bowels; but certainly cold is not the ordinary cause of these movements. At first, they may exist in a warm atmosphere, and then they appear to be more rapid than in a cold atmosphere. Besides, the bowels may be exposed to a cold atmosphere, and remain motionless, although they have their entire irritability. As to atmospheric air, it is not able to excite a

movement in the bowels. If we open the abdomen of a living animal, in avoiding to excite mechanically the bowels, and in allowing the animal to breathe freely, we may for a long time see no other movement in the bowels, except, sometimes, slight regular and natural peristaltic motions, depending on digestion, and limited to some small parts of the bowels. The animal must be kept on his back, and we must avoid touching the bowels, because a slight contact is sufficient to produce movement. Now, if we prevent the animal from breathing, we see, after ten, fifteen, or twenty seconds, very violent, sudden, and rapid contractions taking place in all parts of the intestine, from the stomach to the rectum, but much more in the small intestine than elsewhere. These movements are quite different from the digestive peristaltic movements. If the animal is allowed to breathe again, and freely, the movements diminish gradually, and disappear almost entirely after a few minutes. Then, if we prevent it again to breathe, we see the movements produced again. This experiment may be repeated many times, with the same result, on the same animal.

We are certainly entitled to conclude that there is an exciting cause of contractions, developed during asphyxia, and that it is neither the cold nor the atmospheric air which produces in all cases the movements of the bowels after the opening of the abdomen. We may draw the same conclusions from another experiment. If we put a tie around the trachea of a living animal, immediately after expiration, we may see and feel violent movements taking place in the bowels, although the abdomen is not opened. It is in consequence of such movements that there is an expulsion of fæcal matters, after death, in man. The urine may be also expelled in these cases, in man and in animals, and this expulsion takes place because the bladder contracts, and not, as it is generally admitted, because the *sphincter vesicæ* becomes relaxed.

Some physiologists have considered the cessation of the circulation of the blood in the bowels as the cause of their movements, after death, and they relate as a proof the fact that the section of one of the arteries going to a part of the intestines, is followed by contractions in the parts thus deprived of circulation. But nothing is explained by saying that the cause of the contraction is

in the absence of circulation. As contractions require an excitation to be produced, what is the exciting cause when the blood does not circulate? After the section of an artery there is blood remaining in the capillaries, and that blood, after a short time, becomes very rich in carbonic acid, and then, if my theory is right, contractions ought to be produced. The result of the section of one of the arteries is, therefore, in accordance with my theory.

Other facts may be adduced proving the influence of black blood and carbonic acid on the bowels.

If black blood is injected in the arteries of the small intestine when its irritability is much diminished, movements are almost immediately produced, but they do not last long. On the contrary, if red blood is injected, movements do not appear immediately, and they are very strong and last long. This action of red blood may be easily understood: it increases the irritability of the muscular layer of the bowels, as it does for that of the muscles of animal life, and when it has been changed into black blood, it excites the muscular tissue and produces contraction. The strength and the long duration of the contraction in this case depend on the increase of irritability. When, as in the above experiment, black blood (containing a great quantity of carbonic acid, on account of the constant formation of that gas in blood deprived of the contact of atmospheric air) is injected, the irritability is not sensibly increased, but the excitation is considerable and there is an almost immediate effect.

If air is injected in the arteries of the bowels, soon after the death of the animal, a part of the blood it contains is expelled, and we find that the movements do not last as long as if the blood had not been removed.

When an animal is killed by hæmorrhage, the intestine, as well as all the other organs, contains more blood than usual, and then its movements are not so strong, and last less than they do generally.

When in a recently asphyxiated animal the arteries and veins of a part of the bowels are divided, the movements of that part become less strong and last less than those of the other parts of the intestine.

When the bowels of a recently asphyxiated animal are put under a receiver containing carbonic acid, their movements are

very much increased, but they do not last so long as when they are in the atmosphere.

When they are put under a receiver containing hydrogen, their movements are very quickly diminished in strength, and they last still less than when exposed to carbonic acid.

When they are put in oxygen, their movements diminish a little at first and soon after become stronger, and they last much longer than usual.

As a general conclusion about the apparently spontaneous contractions which I have described as taking place in paralyzed muscles during life or after death, I will say that it seems that black blood by its carbonic acid is the cause of these contractions. When the nervous centres are still united with the contractile tissues, we see, during agony or after death, stronger movements generally than when they are separated. The action of black blood on the nervous centres may be very great. I have found that the spinal cord, when separated from the encephalon, may be strongly excited by black blood. If an animal is asphyxiated after a transversal and complete division of its spinal marrow in the dorsal region, we see convulsions taking place in the posterior limbs, and they are nearly as strong as when the nervous centers have not been injured. The excitation on the spinal marrow is considerable enough to produce an erection of the penis.*

XXXIV.—ON THE CAUSE OF THE BEATINGS OF THE HEART.

The cause of the rhythmical movements of the heart has been heretofore unknown. I believe I have discovered it.

Before exposing my theory and the facts upon which it is grounded, I will show that the theories put forward until now are not correct.

There are three theories only which are worthy of examina-

* Almost all, if not all, the secretions of the body are increased during asphyxia: bile, (as shown by Professor Bouisson,) saliva, tears, gastric, pancreatic and intestinal juices, and also liver-sugar, etc., are produced in greater quantity then than usual. I believe that this increase results from the excitation of the nervous system, and, in some measure, perhaps, from a direct action of black blood on the capillaries of the glands. The urinary secretion may also be changed in asphyxia, and not only then the urine may contain sugar, as Alvaro Reynoso has found, but also albumen.

tion: 1st, that of Haller; 2d, that of Carpenter; 3d, that of Budge, Schiff, and others.

Haller has been very near the truth in admitting that the beatings of the heart were excited by the blood. His error has been an *error loci.* He thought that the blood acted in the cavities of the heart. It is not so; and it is known that the heart may continue to beat after all the blood has been drawn out of its cavities.

The doctrine of Carpenter* is a very simple and remarkable one. He believes that the muscular fibres may act without having been excited. A muscle, says he, may be compared to the electric jar, and become so *charged* with *motility*, (or motor force,) as to execute spontaneous contractions; and elsewhere, "It is not very difficult to conceive that the ordinary rhythmical movements of the heart may be due to a simple excess of this motility, which is continually being supplied by the nutritive operations, and is as constantly discharging itself in contractile action." Carpenter believes that the reason for which the heart presents spontaneous contractions while the other muscles do not, (at least ordinarily,) is, that there is a higher degree of motility in the heart. He considers as very important the facts I have discovered, that many other muscles besides the heart may present rhythmical movements. He thinks that these facts show there is a tendency to rhythmical movements in the muscles themselves, altogether independent of the excitement to action which they receive through the nervous system.

The best ground for the hypothesis of Carpenter is that, according to him, the heart continues to beat, although it is not exposed to any excitation in certain circumstances. He says: "When every source of excitement is excluded, we cannot but perceive that these actions take place with a *spontaniety* which can scarcely be accounted for in any other way than by considering them as expressions of the vital activity of the component cells of these forms of muscular tissue, which manifests itself in this mode, when the developmental life of the cell has attained its maturity. And this view is strikingly confirmed by what we know of the origin and termination of these movements.

* See his *Principles of Human Physiology, American edition,* by F. G. Smith. Philadelphia, 1853. pp. 130 to 132, 319, 325, and 471–72.

For the action of the heart commences when, as yet, its contractile parietes consist but of an assemblage of ordinary-looking cells, no proper muscular tissue being evolved, and no nervous system being yet developed, from which the stimulus to the movement can proceed; and *it is impossible to assign any other cause for the movement under such circumstances*, than the attributes inherent in the tissues which perform it."

The first thing to be said against the view of Carpenter is, that his hypothesis is not necessary; because *it is possible to assign another cause for the movement of the heart under the circumstances* he speaks of. This will be proved hereafter.

The doctrine of Carpenter implies, that the degree of irritability (motility, motor force, contractility,—never mind the name) is greater in the heart than in the other muscles which have no spontaneous action. This is not the case. The degree of irritability, as judged by its duration after death, is generally greater in the muscle of animal life, than in the heart. The accepted sentence of Haller, *Cor ultimum moriens*, generally, is not true.

If Carpenter was right, we should see, during life, the apparently spontaneous contractions which take place in all the contractile tissues after death; because their irritability is at a higher degree in the first, than in the second case. Besides, we should not see oxygen, or red blood, diminish the frequency of the beatings of the heart; and black blood, or carbonic acid, increase that frequency.

An experiment, consisting in the research of the influence of *vacuo* on the heart, has been made by Tiedemann and by Dr. S. W. Mitchell, and Dr. T. H. Bache, (see Dunglison's Physiol., vol. ii. p. 150.) It seems to me that the result of this experiment is in complete opposition to the doctrine of Carpenter. These experimenters have found that the beatings of a heart were speedily brought to a stand by the exhaustion of the air, and that they were renewed when it was re-admitted. If the view of the eminent British physiologist was right, we ought to see the heart continue to beat in vacuo about the same length of time as it would in hydrogen or nitrogen, because its irritability cannot be suddenly diminished enough by the exhaustion of the air. In these gases the heart of a mammal may beat for five or

ten minutes or more, and the right auricle may beat for hours; and the heart of a frog may beat for one day. It is much more to account for the stopping of the heart's action in admitting that the excitant of that action is removed during the exhaustion of the air. John Reid had found that the heart of a frog had continued to beat *in vacuo*, but how long he does not say.*

I will relate hereafter many experiments of mine which are in opposition to the theory of Carpenter.

It is one of the most important questions in physiology, whether the nervous centres, the nerves, and the contractile tissues are able to act without stimulation. This question has not been yet entirely treated by any physiologist. I propose publishing a special paper on the subject. I will merely say here that there may be apparently spontaneous actions in the spinal cord, as well as in the muscles. For instance, very frequently, in a frog, after the removal of the brain and the medulla oblongata, we may see strong movements apparently spontaneous, but when we know that the slightest excitation of the skin, or of any other very sensitive part, may excite the spinal cord, and produce a reflex action, we are authorised to consider all the movements taking place as reflex actions. An excitation may have come to the spinal marrow from the bladder, from the bowels, from the lungs, (in which worms are almost always found in the cold seasons, i. e. at the time these phenomena are generally observed,) etc.

As to the spontaneity of action in muscles, I have tried to prove in a preceding article that it is a mere and false appearance.† I will prove hereafter that the cause of the apparently

* Art. Heart, in Todd's Cyclop., vol. ii. p. 611. J. Reid says in the same page, "We ought to be more cautious in admitting the existence of this innate moving power, since it is in opposition to a well known law in the animal economy, that though the various tissues of an organised body are endowed with certain vital properties, yet the application of certain external and internal stimuli is necessary to produce their manifestations of activity. In fact it is from the action and reaction of these tissues and excitants upon each other that the phenomena of life result."

† Carpenter says that the action of the uterus, as it shows itself, "not merely in the final parturient effort, but in local contractions that frequently occur during the latter months of gestation, (simulating the movements of the fœtus,) are more satisfactorily accounted for by considering them as a discharge of accumulated power, than in any other mode." I will try to

spontaneous contractions of the heart, is the same as that of the like contractions in other contractile tissues.

The physiologists who maintain that the beatings of the heart depend on the nervous system, appear to me to be greatly mistaken. They make a confusion between two things, greatly distinct, one from the other: they conclude from the fact that the nervous system is able to act on the heart, that its influence is necessary. It is the same kind of mistake which is so frequently made as to the influence of the nervous system on nutrition, on secretions, and on animal heat; because that system is able to act upon these functions, it is concluded that its influence is necessary.

The first argument to be adduced against the writers who admit, as necessary, the influence of the nervous system on the heart, is, that they change only the ground of the difficulty in doing so. Instead of having to explain why the heart acts rhyth-

prove elsewhere that for the uterus, as well as for any other contractile tisue, there is no spontaneous action. The uterus, in pregnancy, becomes more and more irritable every day, and when its irritability has arrived at a very high degree, then the slight excitation produced by the carbonic acid normally contained in the blood is sufficient to put it in action. When the contractions have begun, they are very much increased by a reflex action. Every contraction is accompanied by a galvanic discharge on the nerves in the neighborhood of the muscular fibres which contract, and the sensitive nerves being thus excited, it results, 1st, that a pain is felt, the degree of which is in proportion to the degree of any contraction, and therefore with the degree of galvanic discharge;* 2d, that the spinal marrow is excited, and produces reflex movements in the uterus. Now, the more these reflex contractions are energetic, the more they are induced to take place again, on account of the galvanic discharge which accompanies them. So that there would be a constant increase in the intensity of the contractions if there were not four limits to them. 1st, there is no galvanic discharge when the muscular fibres *are contracted;* it is only at the time they *are contracting* that this discharge takes place; 2d, the primitive cause of contraction, the excitation of the muscular tissue, by carbonic acid, diminishes much during the contraction, because the caliber of the small blood-vessels is much diminished, and the blood expelled from them; 3d, every contraction of the uterus diminishes the degree of its irritability; 4th, the reflex power of the spinal cord becomes exhausted, or at least diminished.

* See, on this subject, my paper in the Comptes rendus de la Société de Bologne, en. 1850, t. ii. p. 172.

mically, they have to explain why the nervous system acts rhythmically on the heart. Not only they have not explained this rhythmic action of the nervous system, but, as far as I know, they appear not to have been aware that this was to be explained.

The second reason I will mention, is the fact, so well established by my friend Professor Lebert, that, in embryos, the heart beats when it is merely composed of cells, and when the nervous system has not yet appeared.

A third reason is, that, either in monsters, or in animals operated on by physiologists, there has been a long persistence of the beatings of the heart when a part of the cerebro-spinal centre did not exist, or had been removed. Any part may be in that case, even the medulla oblongata, as I have discovered. (See Art. xvi. p. 40.)

In opposition to the idea that the beatings of the heart depend on the microscopical ganglia existing in that organ, I will say, that, besides the fact that the heart beats in embryos before the nervous system exists, and besides the improbability that such a small amount of nervous matter should have so great a power, there are two good reasons against this strange theory:—1. There have been found no ganglia, large or microscopical, in the auricles, in the sinuses of the pulmonary veins, or in those veins. All these parts, nevertheless, may continue to beat a long while, (even for hours,) after they have been separated from the ventricles where are the microscopic ganglia. 2. Rhythmical movements may exist in a great many other muscular parts of the body, where there is no microscopical ganglion, and where these parts have ceased to be under the influence of the nervous centres.

The three theories which I have examined being unable to explain the beatings of the heart, I will now expose my theory, and discuss the three following questions:—1. What is the excitant which puts the heart in action? 2. Does that excitant act rhythmically? 3. Does that excitant act together directly on the muscular fibres of the heart, and on the nervous system; or does it act only on the muscular fibres?

After having solved these three questions, I will examine the objections which might be made to the doctrine I propose.

1. *What is the excitant which puts the heart in action?*

I believe that the beatings of the heart are excited by a prin-

ciple existing in the blood, and that carbonic acid is that principle. This view is grounded on the following facts:

a. When we prevent a warm-blooded animal from breathing, the beatings of the heart become more frequent than before, for about one or two minutes. It is not on account of the emotion alone that it is so, because the same effect is produced when we asphyxiate suddenly an animal which has entirely lost his power of having emotions, in consequence of the action of chloroform.

b. Many times I have found, on myself and on one of my friends, that the beatings of the heart are rendered more active during asphyxia. We hold our breath for about three quarters of a minute, and during the last fifteen seconds the heart beats from two to four (in one case five) minutes more than when the respiration was free. We have made the experiment in the sitting position, avoiding any movement of the body in all the cases.

c. John Reid has discovered that when any hemadynamometer is put in the femoral artery of a dog, the mercury rises in the instrument if the animal is asphyxiated, and about one minute after the respiration has been stopped. The same result has been obtained in twenty experiments. It seems to me that this fact proves that the contractions of the heart become more energetic during asphyxia. John Reid attributes the result he has obtained to some difficulty that black blood seems to have in passing through the capillaries of the different parts of the body. I do not deny that there is such a difficulty; but I think that the great reason of the ascension of mercury in the hemadynamometer is, the increase in the force of the heart. A simple experiment proves that I am right. I adapt the hemadynamometer to the aorta in the abdominal cavity, and then I open quickly the chest, and I put a ligature to the brachial and carotid arteries. About three quarters of a minute after opening the chest, and about half a minute after the ligature has been put on the arteries of the head and arms, the mercury rises notably in the instrument; sometimes the elevation is as considerable as two inches. It results from this experiment, that the heart beats more strongly in asphyxia about one minute after its beginning.

d. Woodall, a most intelligent and accurate observer, says Dr. Martin Paine, (see Med. and Physiol. Comment., t. ii. p. 49,)

states, that the best remedy for syncope is to obstruct respiration entirely by momentarily confining the nose and mouth. If this be true, it is in perfect accordance with my view, that, during asphyxia, the normal cause of the beating of the heart increases in the blood.

e. If a frog is put under a receiver containing pure oxygen, at a temperature of 40 or 50° Fahr. (4, 5, or 10 Cent.) after its heart has been laid bare and its central nervous system destroyed, we see the heart beat for a very long time, (one, two, or three days.) On the contrary, if, at the same temperature, another frog, deprived also of the central nervous system, is put in carbonic acid gas, the heart beats very quickly at first, but it soon ceases to beat, (in one or two hours only, sometimes, and for the most about half a day.)

f. All the causes which increase the formation of carbonic acid gas in the body, increases the frequency of beatings of the heart.

g. If we inject the serum of blood into the arteries of the heart, so as to expel as completely as possible the blood contained in the capillaries of this organ, and if then we remove the blood from the cavities of the heart, we find that its beatings are, at once, almost entirely suspended, and that they are completely stopped in a very short time, (from one to eight minutes.) The muscular irritability is not destroyed in this organ; it does not beat because its excitant has been removed.

h. I have found that when the heart of a young animal is put in hydrogen, its beatings hardly change at first, but they stop in a very short time. When it is put in carbonic acid gas, its beatings are, at first, increased in frequency and strength; but they very soon are stopped. When it is put in oxygen, its beatings are slowly increased in frequency and strength, and they last very long.

i. On newly-born cats and dogs, before the occlusion of the *ductus arteriosus*, I open the chest and put a ligature on the arteries going to the head and fore-limbs, and on the aorta immediately after the origin of the ductus arteriosus. Then the blood, expelled from the right ventricle, is sent to the lungs, from which it comes to the left auricle, and afterwards to the left ventricle. From there it is sent into the only part of the aorta remaining accessible, and thence it goes into the cardiac arteries, and into the

pulmonary artery, through the ductus arteriosus, (a direction which is the reverse of the normal direction in that duct.) By the cardiac veins the blood arrives again in the right side of the heart. The circulation from the heart to the lungs, and *vice versa*, continues very well. I have found, that if hydrogen is insufflated into the lungs, the beatings of the heart are not much changed at first, but they go on diminishing, and they disappear in a short time. When an injection is made with carbonic acid, the beatings of the heart are quickly increased in frequency and strength; but they are stopped after a short time. When oxygen is insufflated, the beatings of the heart become slowly more frequent, and they remain quick and strong for a long time. (I have once, by such insufflation of oxygen, maintained beating for eleven hours in the heart of a young cat.)

I believe that these facts prove that black blood, by its carbonic acid, is an excitant of the beatings of the heart. If, now, we adduce to these facts all those I have related in a preceding article, on the apparently spontaneous contractions in all the contractile tissues of the body, we shall have a very considerable number of facts, proving that, during asphyxia, there is an accumulation in the blood of the principle which causes these contractions. I believe that it is almost impossible to deny that this principle is the carbonic acid gas.

Before trying to show that what takes place in asphyxia in the heart is only an exaggeration of what normally exists in that organ, I will treat the two remaining of the three questions I have announced I would endeavor to solve, as regards the excitant of the heart's action.

2. *Why does that excitant act rhythmically?*

I believe it is easy to explain why the agent of excitation of the heart* produces rhythmical contractions. I will suppose, first, that the action is permanent. A part of the heart, ventricles, or auricles, being dilated, receives an excitation in all its fibres simultaneously, and a contraction is produced. But, according to the well-known law of Schwann, the exciting cause which is able to give the impulse when the muscular fibres are long, is not able

* What I will say here for the heart, might be said for all the contractile tissues, presenting apparently spontaneous rhythmical contractions, as the cilia, for instance.

to maintain the contraction when the fibres have been shortened. Then, on account of this insufficiency of power of the cause of the contraction, a dilatation ensues. We may present the fact in other words, and say that the resistance to the contraction originating from the displacement of the constitutive matter of the contractile tissues, increases in proportion to the shortening of the fibres; and that after the fibres have contracted under the impulse of the exciting cause, although this cause continues to act, a dilatation is produced by the force belonging to that resistance, which is nothing but elasticity. If the cause of the contraction of the heart was a considerable one, then we should see a permanent contraction; and it is so when we apply galvanism—the elasticity, then, is not powerful enough to produce dilatation. On the contrary, with a weak exciting cause, like carbonic acid, the result ought to be different. When that cause has more power, as in asphyxia, the shortening of the fibres takes place quicker, and is more considerable; and even then it is not sufficient to maintain contraction, the tendency to dilatation being also increased.

I ought to say, that the excitant cause of the contractions is not always at the same degree of power. The small blood-vessels and the capillaries being compressed during the muscular contractions, there is a diminution of excitation during that time. This should be sufficient to explain the alternate contractions and dilatations. But such a diminution in the caliber ought to be very little, if even it exists in certain organs, (the heart when composed of cells, for instance.)

I come now to the third question about the excitant of the heart—

3. *Does that excitant act together on the muscular fibres and on the nerves of the heart, or does it act only on the muscular fibres?*

I believe it ought to act also on the nerves; but I cannot prove it otherwise than by saying, that all the agents of excitation that we know to act on the muscular fibres, are able to act on the nerves.

There are many things to be said besides the above facts and reasoning, to prove the truth of the doctrine I propose. I will expose some of them.

The following question might be made:

How is it that the heart is the only muscle containing striated fibres, which presents normally rhythmical movements?

The answer to this question appears to be very simple. The intensity of the stimuli, the degree of irritability, and the resistance which a muscle has to overcome when it contracts, are three elements which we ought not to lose sight of when we examine the difference of contractions between two muscles. Suppose the heart possessing the same degree of irritability as another muscle: if the stimulus is the same, and the resistance the same also, for the heart and for the other muscle, there will be the same effects. But if the stimulus is more considerable in the heart than in the other muscle, and if the resistance to be overcome is less for the heart, then with the same degree of irritability in both parts, and even with less irritability in the heart than in the other muscle, we will see a movement in the heart, and not in that other muscle. Now a simple examination of the vessels of the heart, proves that they contain more blood, and consequently more stimulus, than the other striated muscles. Besides, as the heart is not inserted into heavy bones to be moved, it has less resistance to overcome when it has not to circulate the blood, as after death, or when it is out of the chest, than the muscles of animal life. Some muscles in the face and the diaphragm, being almost without an external resistance, when their contractions do not go so far, it results that they are moved much more easily after death, than the muscles of the limbs. In consequence of these views, I believe that, although there is in the blood-vessels of all the muscles of the body a principle which is an exciting cause of contractions, there are no contractions produced, because the quantity of that principle is not sufficient, or because the resistance to contractions in many muscles is greater than in the heart.

I must, in conclusion, say, that I do not advance my theory of the rhythmical movements as perfectly proved. I believe it is true, and that there are a great many facts which appear positively to prove it. What I can assert is, that it is by far much more in harmony with all the known facts, than the other theories.

Mortality of Philadelphia, for April, May and June, 1853, *arranged from the record kept at the Health Office.* By WILSON JEWELL, M. D.

The deaths from all causes reported during the quarter ending July. 2d, embracing a period of 91 days, amount to 2235. Compared with those for the same period of last year, there has been a falling off of 15 per cent. in the aggregate.

Deducting the "Still Born," amounting to 150, the deaths from "External Causes," 82, "Old Age," 29, and "Unknown," 46, amounting in all to 307, and there will remain 1928 deaths from known diseases.

The number of deaths reported under one year of age, exclusive of still-born children, amounts to 482, or nearly one-fourth; and those under five years amount to 915, or about one half of the whole number of deaths from known diseases.

The deaths among males were 1157, females 1078—being an excess of 79 of the former over those of the latter.

Estimating the population at about 415,000, the aggregate of deaths in the quarter will give 1 in every 181½ of the population; which is equivalent to 25 deaths per day, during the quarter.

Eleven hundred and thirty-two, or about fifty per cent. of the whole number of deaths recorded, were under 20 years of age.

CLASS 1st.—Among this class, the "Endemic or Zymotic," there is a decrease of 162, or about 25 per cent. from those for the like period in 1852. The difference is accounted for by the falling off in small-pox and measles. Last year the mortality from these diseases was wide-spread. The present year, thus far, they are comparatively rare.

CLASS 2d.—Of the diseases of "Uncertain Seat," there were 297 deaths. Of these, 212 are charged to Debility, Marasmus and Dropsy, of which number 103 were under one year of age.

CLASS 3d.—The deaths under this head, "Nervous System," amount to 393. 142, or nearly one-third, were from Convulsions, 47 from Dropsy of the Brain, and 40 from Inflammation of the Brain. Of these, 114 were under one year of age.

Class 4th.—The diseases of the "Organs of Respiration," amounting to 515, constitute nearly one-fourth of the whole number of deaths for the quarter.

Consumption of the Lungs make up a large proportion of this class—namely, 312; while Inflammation of the Lungs furnish 103. The deaths from Consumption of the Lungs are less 12 than for the same period in 1852. 92 of the deaths took place between the ages of 20 and 30. The deaths among females exceed those of males by 11 per cent.

Class 8th.—The diseases of the "Organs of Generation" furnish only 22 deaths. Five of these are charged to Child-bed and four to Puerperal Fever. In 1852, in the same period, there were 24 deaths from diseases of the Organs of Generation, eight of which were from Puerperal Fever.

Class 12th.—From "External Causes" there were 82 deaths. 56 of these occurred in males and 26 among females. The exposed situation of the male sex over that of females, and the danger of their out-door employments, may add to the increase of deaths among males from external causes.

Table No. 3, furnishes the exact number of deaths at fifteen distinct periods of life. By this it will be seen that infancy furnishes a large proportion of the deaths; that between birth and the fifth year, 915 children have perished during the past three months—a number almost equal to one-half the whole number at every age of life. The healthiest period of childhood lies between the ages of 10 and 15, and this corresponds with other statistics.

The deaths that took place at the Alms-House during the quarter, were 174, and those which occurred among people of color were 190.

TABLE No. I.

Deaths for the Second Quarter of 1853, *classified.*

	April.	May.	June.	MALE. A.	M.	J.	FEMALE. A.	M.	J.	Total.
1. *Endemic and Contagious Diseases.* Zymotic or Epidemic . .	135	108	218	59	49	112	76	59	106	461
2. *Uncertain or general seat.* Sporadic diseases	94	76	127	55	36	63	39	40	64	297
3. Nervous System . . .	140	104	149	77	52	89	63	52	60	393
4. Organs of Respiration . . .	203	152	160	99	83	78	104	69	82	515
5. " Circulation . . .	24	17	23	12	6	8	12	11	15	64
6. Digestive Organs . . .	54	41	56	31	16	33	23	25	23	151
7. Urinary Organs . . .	1	2	6	1	2	5			1	9
8. Organs of Generation . .	6	11	5				6	11	5	22
9. " Locomotion . .	6	5	1	1	2		5	3	1	12
10. Integumentary System . .	1	2	1	1	1	1		1		4
11. Old Age	14	6	9	6	3	6	8	3	3	29
12. External Causes	19	26	37	11	17	28	8	9	9	82
Still Born	50	33	67	31	21	36	19	12	31	150
Unknown	9	14	23	6	5	15	3	9	8	46
	756	597	882	390	293	474	366	304	408	2235

TABLE No. II.

1. *Endemic and Contagious Diseases.—Zymotic or Epidemic.*

	Male.	Female.	Under 1 yr.	1 to 2.	2 to 5.	5 to 10.	10 to 15.	15 to 20.	20 to 30.	30 to 40.	40 to 50.	50 to 60.	60 to 70.	70 to 80.	80 to 90.	90 to 100.	100 to 110.	Total.
Cholera Infantum .	28	35	53	8	2													63
" Morbus .	2	2	2						1			1						4
Croup . . .	25	29	7	8	30	9												54
Diarrhœa . . .	17	12	14	3	1				4	2	1		1		3			29
Dysentery . .	31	31	18	12	15	1		1		2	2	2	5	1	2	1		62
Erysipelas . .	14	12	10	1	2		1	1	2	2	1	3	2		1			26
Fever . . .	2	4	1						1	1	1	1	1					6
" Bilious . .		4		1	1			1			1							4
" Continued .		3								2		1						3
" Intermittent .		3			1					2								3
" Remittent .	5	7		3				2	2	3		1		1				12
" Scarlet . .	37	38	4	13	34	19	4	1										75
" Typhus . .	26	23				1	1	4	12	12	6	8	2	2	1			49
" Typhoid . .	19	23			1	5	4	3	6	5	7	7	1	1	1	1		42
Hooping Cough . .	3	4	2	5														7
Influenza . . .		1		1														1
Measles . . .	3	2	1	1	3													5
Small Pox. . .	7	8	7	1	2	2		1	1	1								15
Syphilis . . .	1									1								1
	220	241	119	57	92	37	10	14	29	33	19	24	12	5	8	2		461

2. *Uncertain or General Seat.—Sporadic Diseases.*

	Male.	Female.	Under 1 yr.	1 to 2.	2 to 5.	5 to 10.	10 to 15.	15 to 20.	20 to 30.	30 to 40.	40 to 50.	50 to 60.	60 to 70.	70 to 80.	80 to 90.	90 to 100.	100 to 110.	Total.
Abscess	4			1				1	1	1								4
" Neck		1		1														1
" Psoas	1	1										1	1					2
Angina		4	2			2												4
Cancer	2	3					1					4						5
" Breast		1										1						1
Cachexia	1	1			1								1					2
Cyanosis	2	3	5															5
Debility	45	50	40	3	2		1	1	3	9	3	7	11	9	6			95
Disease of Throat	1										1							1
Dropsy	15	11	4		5	2		1	3	1	1	1	4	2	1	1		26
Fungous Glands of Neck	1												1					1
Gangrene		2		1	1													2
Glandular Swelling		1	1															1
Hectic Fever	2								1	1								2
Hemorrhage	3	5	4		2				1				1					8
Inanition	5	3	5		1						1	1						8
Inflammation	3				2						1							3
" Throat		1			1													1
" Tonsils	1			1														1
Malformation	1	3	3	1														4
Marasmus	44	47	59	11	14		2	1		1	3							91
Melœna	1	1	1								1							2
Scirrhus	1											1						1
Scrofula	9	3	6	1	1	1		1		1			1					12
Spina Bifida	1	1	2															2
Tabes Mesenterica	7	1	6	1	1													8
Ulceration	2									1		1						2
" Throat	2			1						1								2
	154	143	138	22	31	5	4	5	9	16	11	17	20	11	7	1		297

3. *Of the Nervous System.*

	Male.	Female.	Under 1 yr.	1 to 2.	2 to 5.	5 to 10.	10 to 15.	15 to 20.	20 to 30.	30 to 40.	40 to 50.	50 to 60.	60 to 70.	70 to 80.	80 to 90.	90 to 100.	100 to 110.	Total.
Abscess of Brain	1					1												1
Apoplexy	12	11			1			1	1	1	5	4	6	4				23
Cerebral Fever		1			1													1
Compression of Brain	1		1															1
Concussion "	2	1		2						1								3
Congestion "	19	14	11	4	8		1		4	2	1	1		1				33
Convulsions	69	73	80	19	25	3	4	1	1	5	2	1	1					142
" Puerperal		1								1								1
Coup de Soleil	20	4		1		1		3	7	5	5	1			1			24
Cramp	1		1															1
Dropsy of Brain	23	24	23	12	8	3					1							47
Effusion "	6	8	4	1	3	1		1	1			1	2					14
Epilepsy	3	3	1					2	3									6
Neuralgia	1	1							1				1					2
Palsy	6	7							1		2	3	2		5			13
" of Brain	1				1													1
Mania a potu	19	1						1	4	4	6	3	2					20
Softening of Brain	1	1									1				1			2
Tetanus	3		1						1		1							3
Trismus	1	1	1	1														2
Tumors of Brain	1							1										1
Disease "	6	6	2	1	2			1	1	2	3							12
Inflammation "	22	18	11	9	11	4	2		1	1		1						40
	218	175	136	50	60	13	7	11	26	22	26	16	14	5	7			393

4. *Organs of Respiration.*

	Male.	Female.	Under 1 yr.	1 to 2.	2 to 5.	5 to 10.	10 to 15.	15 to 20.	20 to 30.	30 to 40.	40 to 50.	50 to 60.	60 to 70.	70 to 80.	80 to 90.	90 to 100.	100 to 110.	Total.
Abscess of Lungs	1	3			1		1					2						4
" Thorax		1	1															1
Asthma		3								1			2					3
" Thymic		1					1											1
Consumption of Lungs	147	165	3	4	3	5	9	30	92	74	50	20	16	6				312
Congestion "	4	5	3	3					1				2					9
Disease of Lungs	8	1	3	1		1			3		1							9
Dropsy of Chest	4	6		1	2		2	1		1			1	2				10
Effusion "	2	1			1	2												3
Empyema	1								1									1
Disease of Chest	1					1												1
Effusion of Lungs		1												1				1
Hemorrhage of Lungs		4	1							1	1		1					4
Inflammation "	61	42	16	9	20	5	1	2	14	11	5	7	5	7	1			103
" Larynx	5	1	2		1			1	1	1								6
" Bronchiæ	19	16	13	4	5				2	1	1	3	3	3				35
" Pleura	2	2							3			1						4
" Chest	2	1		2	1													3
Palsy of Lungs	1				1													1
Ulceration of Larynx	1	1		1					1									2
Tuberculosis	1	1					1					1						2
	260	255	42	25	35	14	15	34	118	90	58	34	30	19	1			515

5. *Organs of Circulation.*

	Male.	Female.	Under 1 yr.	1 to 2.	2 to 5.	5 to 10.	10 to 15.	15 to 20.	20 to 30.	30 to 40.	40 to 50.	50 to 60.	60 to 70.	70 to 80.	80 to 90.	90 to 100.	100 to 110.	Total.
Angina Pectoris . .		1										1						1
Anæmia . . .	1	1			1						1							2
Dropsy of Heart . .	3	6	2		1	1	1			1	1		1			1		9
Disease " . .	17	23	4	4	3	1	2	1	2	6	6	5	2	2	2			40
Enlargem't " . .	1	5		1				1	1		1	1		1				6
Inflammation " . .	4	1						1	1		2	1						5
Ossification " . .		1												1				1
	26	38	6	5	5	2	3	3	4	7	11	8	3	4	2	1		64

6. *Digestive Organs.*

	Male.	Female.	Under 1 yr.	1 to 2.	2 to 5.	5 to 10.	10 to 15.	15 to 20.	20 to 30.	30 to 40.	40 to 50.	50 to 60.	60 to 70.	70 to 80.	80 to 90.	90 to 100.	100 to 110.	Total.
Abscess of Liver .	1	1							1	1								2
Abdominal Dropsy .	3	3							2	1		1	1	1				6
Cancer of Stomach .	1										1							1
Cancrum Oris . .	3	2		1	2	1	1											5
Cirrhosis . . .	1	1											2					2
Consumption of Bowels	1	1				1			1									2
Disease of Bowels .	1	1			1							1						2
" Liver . .	2	1	1							1	1							3
" Stomach .		2							1	1								2
Dyspepsia . . .	1			1														1
Enlargement of Liver .	1								1									1
Gastric Fever . .	1			1														1
Hernia, Strangulated .	1		1															1
Intussusception . .	1				1													1
Inflammation of Bowels	2	2	2			2												4
" Liver . .	7	3			2		1	1		2	2	1	1					10
" Stom'h & Bow'ls	32	29	12	12	10	1		3	9	3	5	2	1	1	1	1		61
" Peritoneum .	10	13	1		2		2	2	6	5	1	3	1					23
Jaundice . . .	5	2	3		1	1			1				1					7
Obstruction of Bowels .	1	1								1			1					2
Perforation of Stomach	1									1								1
Scirrhus of Pancreas .		1										1						1
" of Stomach .	2								1		1							2
" of Bowels .		1										1						1
Teething . . .	2	3	3	2														5
Tumour of Abdomen .	1													1				1
Worms . . .		1				1												1
Ulceration of Bowels .		2							1			1						2
	81	70	23	17	19	7	4	6	22	18	11	10	9	3	1	1		151

7. *The Urinary Organs.*

	Male.	Female.	Under 1 yr.	1 to 2.	2 to 5.	5 to 10.	10 to 15.	15 to 20.	20 to 30.	30 to 40.	40 to 50.	50 to 60.	60 to 70.	70 to 80.	80 to 90.	90 to 100.	100 to 110.	Total.
Albuminaria . .	1								1									1
Disease of Bladder .	1													1				1
Inflam. of Bladder .	1	1							1				1					2
" of Kidneys .	1				1													1
Ischuria . . .	1								1									1
Rupture of Urethra .	1									1								1
Stricture . . .	1												1					1
Urinary Calculi . .	1												1					1
	8	1			1				3	1			3	1				9

8. *Organs of Generation.*

	Male.	Female.	Under 1 yr.	1 to 2.	2 to 5.	5 to 10.	10 to 15.	15 to 20.	20 to 30.	30 to 40.	40 to 50.	50 to 60.	60 to 70.	70 to 80.	80 to 90.	90 to 100.	100 to 110.	Total.
Cancer of Uterus . .		3								1	1		1					3
Child Bed . . .		5							1	4								5
Disease of Uterus .		1											1					1
Fever Puerperal . .		4							3	1								4
Hemorrhage from Uterus		3							2	1								3
Inflammation of "		3								2	1							3
Puerperal Mania .		2							2									2
Scirrhus of Uterus .		1											1					1
		32							8	9	2		3					22

9. *Organs of Locomotion.*

	Male.	Female.	Under 1 yr.	1 to 2.	2 to 5.	5 to 10.	10 to 15.	15 to 20.	20 to 30.	30 to 40.	40 to 50.	50 to 60.	60 to 70.	70 to 80.	80 to 90.	90 to 100.	100 to 110.	Total.
Caries . . .		2											1		1			2
Caries of Spine . .	1	1					1	1										2
Congestion Spinal Marrow		1											1					1
Disease of Hip . .		1				1												1
" of Spine . .	2	1		1		1	1											3
Inflam. of Knee Joint .		1											1					1
" of Spine . .		1				1												1
Spinal Irritation . .		1				1												1
	3	9		1		4	2	1					3		1			12

10. ***The Integumentary System.***

	Male.	Female.	Under 1 yr.	1 to 2.	2 to 5.	5 to 10.	10 to 15.	15 to 20.	20 to 30.	30 to 40.	40 to 50.	50 to 60.	60 to 70.	70 to 80.	80 to 90.	90 to 100.	100 to 110.	Total.
Carbuncle		1								1								1
Fistula	1								1									1
Purpura Hemorrhagica	2		1						1									2
	3	1	1						2	1								4

11. ***Old Age.***

Old Age	15	14											3	11	14	1		29

12. ***From External Causes.***

	Male.	Female.	Under 1 yr.	1 to 2.	2 to 5.	5 to 10.	10 to 15.	15 to 20.	20 to 30.	30 to 40.	40 to 50.	50 to 60.	60 to 70.	70 to 80.	80 to 90.	90 to 100.	100 to 110.	Total.
Asphyxia	2	2	4															4
Burns and Scalds	3	7		4		1			2	3								10
Casualties	19	10	4		2	2	2	2	3	3	8	1	1	1				29
Drowned	20	1			1	2	2	2	4	2	4	1	2	1				21
Fracture	1								1									1
" of Spine	1											1						1
Intemperance	5	3							3	2	1	1	1					8
Poisoning		2	1					1										2
Suffocation	1									1								1
Suicide	4	1							1	1	2	1						5
	56	26	9	4	3	5	4	5	14	12	15	5	4	2				82
Unknown	26	20	8	3	3	1	1		6	6	5	6	5	2				46
Still Born	88	62	150															150

TABLE NO. 3.

Deaths for the Second Quarter of 1853, *at fifteen distinct periods of life.*

Under 1 year,	482
1 to 2	184
2 to 5	249
5 to 10	88
10 to 15	50
15 to 20	79
20 to 30	241
30 to 40	215
40 to 50	158
50 to 60	120
60 to 70	109
70 to 80	63
80 to 90	41
90 to 100	6
100 to 110	0
	2085
Still Born	150
Total,	2235

Included in the above Tables, are 174 from the Blockley Almshouse, 190 people of color, and 20 from the country; as follows:

	April.	May.	June.	Total.
Almshouse, . .	65	47	62	174
Blacks, . . .	67	55	68	190
Country, . .	7	5	8	20
	139	107	138	384

A case of Congenital Occlusion of the Vagina, relieved by operation. By GEO. L. UPSHUR, A. M., M. D.; Surgeon to U. S. Marine Hospital, Norfolk; Member of Am. Med. Association; Fellow of Med. Soc. of Virginia, &c., &c.

The subject of this notice was a negro girl, aged 18, the property of Dr. Southall, of Smithfield. She was admitted into the Norfolk Slave Infirmary, October 8th, 1852.

About eighteen months ago, she commenced to have symptoms characteristic of the approaching catamenia, recurring every four weeks with great regularity, and continuing at each return about four days, but without any discharge from the vagina. A round, well defined tumor had gradually appeared above the pubes, becoming larger after every return of the menstrual symptoms, and giving the appearance, when she entered the Infirmary, of one advanced to the fourth month of pregnancy. Eight months ago she began to suffer from distension, and recently the pressure of the tumor upon the rectum and bladder has given her a great deal of pain. There are, also, evident contractions of the uterus, as if it were making a strong effort to get rid of its contents. These symptoms have been so severe and so continuous for the last two months as to unfit her for work, and oblige her to remain almost constantly in the recumbent position. The patient was well grown, and the mamma and external genitals fully developed, the clitoris and labia being of the ordinary size and shape, and the pubis covered with hair.

She stated that she had repeatedly felt the venereal desire, and four times had attempted to consummate the venereal act, but without success. Upon examination I found that the finger would pass into the vagina only about an inch and a-half, being arrested by a firm, thick septum, perfectly smooth, and offering a complete barrier to further ingress. By pressing upon the tu-

mor when the finger was in the vagina, fluctuation could be easily felt. On introducing one finger into the rectum, and a catheter into the bladder, and inclining the former upward, and the latter downward, the finger came in contact with the catheter just behind the terminus of the vagina, nothing appearing to be between them but the coats of the rectum and bladder. The tumor could also be felt from the rectum, sinking low down into the pelvis, and giving decided fluctuation when gently struck.

The introduction of the speculum showed that the vagina was perfectly formed, *as far as it went.* There was no enlargement at its uterine extremity—no *cul-de-sac* formed. It seemed as if nature had lacked for materials to form an entire vagina, but as far as she progressed she had done her work perfectly.

On the 15th of October I performed the operation, in the presence of Drs. Moore, Rose, Robt. Tunstall, Southall, and H. Nash. The bladder and rectum being first evacuated, the patient was placed upon the table, as in the position for lithotomy, except that the hands were not bandaged to the ankles. Chloroform was then administered, and as soon as she became insensible, I introduced the speculum up to the terminus of the vagina, and distended the parts as far as possible. The instrument was held in position by an assistant who, at the same time, made pressure upon the uterine tumor. A second assistant retained a catheter in the bladder, in such a position that I could easily touch it with the forefinger of the left hand introduced into the rectum. Taking the view that in the space bounded by the rectum and bladder, and the vagina and os tincæ, there was nothing but cellular tissue, I desired to have such guides in the operation as would prevent me from wounding any portion of the pelvic viscera, my object being to make a passage up to the os tincæ directly in a line with what would have been the axis of the vagina had it been complete. The tumor above, and the finger and catheter behind and in front, performed the part of such guides.

The operation was commenced by perforating the centre of the obstruction with an ordinary trochar, which was pushed in to the depth of an inch, and the opening widened by a crucial incision with the scalpel, so as to admit the forefinger of my right hand. The speculum was now withdrawn, the finger introduced, and the

obstructing tissue *torn* in every direction as far as I could reach. The dense and unyielding nature of the tissue rendered this part of the operation tedious and difficult, and so fatigued the hand that I was glad to accept the aid of Drs. Moore and Tunstall.

The way being opened as far as we could reach with the finger, the speculum was again introduced, passing in this time about four inches, and being arrested apparently about an inch short of the os tincæ. Pressure being still made upon the tumor, I carefully divided layer after layer with the scalpel, until I reached the os tincæ, which was immediately made known by an abundant flow of a fluid resembling tar in color and consistency, and perfectly inodorous.

The hips being lowered so as to promote the discharge, the patient was left for four hours. At the end of this time thirty-two ounces had been discharged, and the flow was still going on *with regular uterine contractions*, which continued for twelve hours, until every particle of the secretion had come away. The whole quantity amounted to forty-eight ounces, or about three pints. The patient having rallied entirely at the end of eight hours, was put to bed, and ordered half grain of sulph. morph., and perfect quiet to be observed.

Oct. 16th.—Patient rested well last night, and feels tolerably comfortable this morning. Pulse 80, soft and regular; tongue natural, and appetite good. Upon examining per vaginam, I could easily feel the uterus. The organ was larger than it is in the unimpregnated state, and the os sufficiently patulous to admit the end of the finger in other respects. There was nothing remarkable about it. The patient did not complain of pain from the introduction of the finger, nor was there the slightest soreness from pressure over the abdomen. In the evening, finding that no urine had been discharged since the operation, I introduced the catheter, and drew off thirty ounces. Ordered milk and bread diet in small quantities.

17th.—Had a restless night, with slight shivering and thirst. Soreness upon pressure over the whole abdomen, but particularly in the hypogastric region. Pulse 110, moderately full; skin dry; tongue covered with white fur; anorexia; thirst; bowels constipated, and vaginal discharge whitish, and beginning to be fetid. Ordered an ounce of sulph. magnes. to be given at once,

and the vagina to be syringed with warm water dashed with vinegar, three times a day. Warm fomentations to the abdomen.

18th.—Less soreness than yesterday over the abdomen. Vagina very tender to the touch; discharge feculent; bowels moved once; complains bitterly when the syringe is introduced. Continue treatment.

19th.—General symptoms as yesterday. Vagina hot and tumefied; discharge very fetid; great pain upon introducing the finger; urine drawn off night and morning since the day after the operation. Continue treatment.

20th.—Parts very tender, and very little discharge from them. Administered chloroform, and made a minute examination with the speculum. Vaginal surface intensely red, swollen, and bleeding upon the slightest touch. Os tincæ scarcely discernible for the tumefaction around. In the evening there was some increase of fever, and the patient complained of great soreness. I fear the introduction of the speculum has done harm. Ordered fifteen grains Dover's powder at bed-time.

21st.—Patient passed a very restless night. Intense pain over hypogastrium; pulse 112, small and compressible; thirst; dry skin; decubitus on the back, with legs drawn up. Ordered a dozen leeches to the back.

℞. Hyd. Chlorid mit. gr. xii.
Pulv. opii, gr. iv.
Tart. Emet. gr. iv.
M. Pil. 12—S. one pill every two hours.

22d.—No improvement. Bowels constipated; pulse 120; feeble; tongue dry; complains of agonising pain along the sacrum; some tympanites. Continue pills. Ordered an enema of soap-suds and castor-oil, which produced one evacuation, with comparative relief. Hot fomentations around the hips and hypogastrium.

23d.—Fever continues. Discharge from the vagina is *sanious* and fetid; pain in the sacrum somewhat relieved. Added one grain of quinine to each of the pills, and gave them as before. Ordered f.ʒxii. of magnes. citrat., to be taken at two draughts. Injections (vaginal) of pyroligneous acid, laudanum and water. Continue fomentations.

Evening.—Pulse 130, and feeble; pain diminished; tongue

moist, and coated white; discharge from the vagina large and sanious, (I believe this to be the return of the catamenia;) bowels moved once; patient sleeps, and is sweating profusely; when aroused says she feels very weak. Continue treatment, with milk toddy.

24th.—Patient ptyalised; pain greatly diminished; pulse 100, small, but larger than yesterday; no appetite, but partakes freely of milk toddy and soup; sanious discharge still going on. Discontinue medicine.

25th.—All the symptoms better; passed urine to-day *sua sponte*, the first time since the operation; discharge from vagina less, but still sanious.

From this time *Jane* improved rapidly, so that on the 5th of November I was able to make a very thorough examination of the vagina. The lacerations had healed entirely, but there was so much contraction as barely to admit the forefinger up to the os tincæ; the constriction, however, was dilatable, and I introduced a sponge tent. In the course of a few hours this gave so much pain that it was removed by the nurse, but was again introduced on the next day, and borne with less inconvenience.

On the 12th November catamenia again returned, and continued until the 17th, the quantity and color being natural. During the flow, of course the tent was not worn, nor was any examination made. On the 20th I found that the contraction of the upper end of the vagina had increased; it would scarcely permit my forefinger to pass. The sponge tent was continued until she left the house on the 27th. We informed her master that she was not in a condition to go without treatment, and that unless the process of dilatation was continued, there would be a risk of the vagina becoming again closed. When she left the Infirmary, the forefinger, by exercising a moderate degree of perseverance, could be passed up to the os tincæ. The constriction yielding to persistent pressure like spasmodic stricture of the urethra.

On the 27th of April last Jane was again sent to the Infirmary. She stated that, until two months before, she had regularly had her monthly flow, although the discharge was less free, and accompanied with less pain than formerly. She had been under no treatment since I last saw her, the use of the tent being discontinued from the day she left the house. Upon examination I

found it impossible to reach the os tincæ, although there seemed to be an opening up to it. The speculum was introduced, and an attempt to find the opening made with a probe, but without success. No tumor could be felt from the rectum, nor fluctuation through the vagina, and yet I felt sure that there was menstrual blood locked up in the cavity of the uterus.

A second operation was contemplated, but it was deemed expedient to communicate with her master first. Pending the receipt of his reply, the patient was seized with a profuse discharge from the vagina of dark, grumous blood, more fetid than the pus from a rectal abscess. The discharge came on suddenly in the night, May 5th, and continued for five days, with entire relief to all the symptoms. Before any further examination was made a message was received from Dr. Southall, that her services were required at home, and that he would not have another operation performed yet. She was discharged on the 17th May, and I have not heard from her since.

Remarks.—The condition of the patient, when she last entered the Infirmary, is to be accounted for only upon the supposition that after the tents were discontinued, the vagina continued steadily to contract until it entirely closed, so far as the exit of a tolerably consistent fluid was concerned, while the admission of *air* might readily have been permitted. The contraction may have been of such a character as to make the small opening between the uterus and os externum, fistulous or *valvular* in its shape, thus locking up all secretion behind, but permitting a tolerably free ingress from without for the air. A certain amount of fluid having accumulated, an effort would be made to throw it off; this would produce irritation, possibly ulceration in the neighborhood of the constriction, and finally a breaking down of the obstruction, and a free exit to the fluid. There must have been an admission of atmospheric air to account for the excessive fetidness of the discharge.

It is probable that another operation will be required in this case. The first one may be considered, however, as entirely successful, and doubtless there would have been no ultimate difficulty had the process of dilatation been continued after she left the Infirmary the first time, or had she been permitted to remain until the treatment was completed.

The operation in this case is like to that performed in a similar one by Amussat, except that the *tearing* was completed at one operation, whereas in Amussat's case it required three or four *tearings*, at intervals of three days. As his case is one of considerable interest, and bears a close resemblance in many particulars to that just reported, I will give a brief history of it, condensed from the *Medico-Chirurg. Rev.* for October, 1836.

A girl, 15 years of age, was carried from Germany to Paris to consult some of the leading surgeons there. Her mother stated that she had enjoyed good health until about two years before, when she began to suffer severely from periodic attacks of pain in the abdomen, and in the region of the kidneys. At first they were attributed to obstruction of the bowels; but it was soon discovered that this was not the cause, and on examination it was found that there was no trace of any vagina.

When she came under Amussat's notice, in February, 1832, the abdomen was enormously distended; at the lower part was felt a firm, resisting, globular tumor, supposed to be the enlarged uterus. The external parts of generation appeared to be perfectly normal, but on separating the labia there was no vaginal orifice, but simply a concavity corresponding to the position of the orifice, and lined with a smooth mucous membrane. A catheter having, with some difficulty, been introduced into the bladder, it could be distinctly felt by a finger in the rectum, "in consequence of the great thickness of the intermediate parts;" and higher up the finger met with a large, rounded, tense, and fluctuating tumor, occupying the hollow of the pelvis. The nature of the case now became apparent. The urethra, bladder and rectum were tied together by cellular tissue, and the vagina, in at least two-thirds of its length, "mau quait absolument."

The prognosis was considered unfavorable. MM. Magendie and Marjolin thought the only thing that could be done was to perforate the uterus from the rectum, and thus relieve it from its contents. Boyer regarded the case as hopeless, founding his opinion on the history of all similar cases on record. He therefore strongly recommended that nothing should be done. Amussat, however, was unwilling to abandon the case as irremediable. He had but recently seen two cases of imperforation of the vulva, where he had succeeded in re-establishing the vaginal passage,

by gradually tearing asunder the union between the urethra and rectum with blunt instruments and with the fingers. With the consent of the mother and daughter, he proceeded to try the same method in this instance. We used at first a large sized straight sound, pressing it with considerable force against the obstruction in the direction of the vagina "*comme pour faire un trou.*" Having continued this pressure for some moments, he found that there was quite a deep depression left, without laceration or loss of blood; a small piece of sponge was placed in the hollow thus made, and retained by a bandage. Three days afterwards the operation was repeated, with the fingers, the points being laid close to each other and then gradually separated so as to tear the tissues gently. By having a catheter in the urethra and a finger in the rectum the risk of wounding these organs was avoided. When two inches of the canal were gained, he could feel the distended uterus, and, on the 9th of March, he perforated the tumor with a trocar, widened the opening with a bistoury, and let off twelve ounces of dark blood.

In the course of three days, peritonitis set in, with great prostration, tympanites, and a great tendency to stupor. The case was considered very critical, but yielded at last to leeching and mercurialization. The patient ultimately recovered, and when seen by Amussat in July, 1834, two years after the operation, was in fine health, and menstruating regularly. The vagina was very contracted, but adhesion had been prevented by the frequent use of bougies. The true state of the parts, at that time, is described in the following words of Amussat, after consultation with Magendie and Marjolin: "il existait un petit vagin, qui s'etait formé aux depens des petites et meme des grandes levies, la muqueuse ayant eté attirée dans le vagin artificiel par la cicatrice; *et au fond de ce vagin on decouvrait un trou, ou fistule, qui conduisait á la matrice et par lequel l'ecoulement des regles se faisait librement.*" The part of this sentence italicised describes, I believe, the state of the parts in my patient, when she last left the Infirmary. Indeed the whole case bears a very strong resemblance to mine, not only in regard to its history and the condition of the genital organs, but also in regard to the mode of relief adopted.

The danger of fatal peritonitis after the operation for occlusion

of the vagina, in cases of retained menses, is admitted by all surgical writers. Even when no violence is done during the operation to the surrounding tissues, the simple evacuation of the contents of the uterus seems to be inseparably connected with this formidable disease. In Amussat's case, the gentlest means were resorted to, and the whole operation conducted with all the caution and skill which distinguished that celebrated surgeon, and yet the patient barely escaped with her life. M. Capuron reported a fatal case in 1840, and was of opinion "that when any portion of the vagina is wanting, it is imprudent to have recourse to any operation, under the risk of compromising the life of the patient."

In the July No. of *Med. Chir. Rev.* for 1830, Mr. Worthington reports a case in which he "carefully divided with a scalpel a dense cellular structure, of about half an inch in thickness, situated at the orifice of the vagina, and gave exit to about a pound of dark colored fluid." Notwithstanding the abdominal tumor subsided immediately, and the girl felt greatly relieved, peritonitis set in three days after, and she died on the fourth day. Professor Languebec relates a similar case, in which death took place on the fifth day after the operation, and attributes the tendency to inflammation to the long retention of the menses.

Boyer, who has treated at considerable length of the various congenital defects of the female organs of generation, says, "death is the inevitable consequence of the accumulation of the mensus in the uterus, where there is a *complete* absence of the vagina." I presume, then, that such accumulation is proportionably fatal where there is *partial* absence of the vagina. He says, too, that after patients have escaped the wounding of the rectum and bladder during the operation, he "has known them die from inflammation of the uterus and of the adjoining parts."

But notwithstanding the dangers of the operation for retained menses, especially in those cases where there is total or partial absence of the vagina, no surgeon would be justified in refusing to perform it. Death must be the ultimate result if the accumulation is permitted to go on; this can but occur if an attempt is made to relieve by operation, and there is certainly *a chance* of success, of which the patient should always have the benefit.

Cases of congenital occlusion are fortunately very rare. I

speak not now of simple thickening of the hymen, which frequently serves to retain the menses, and is perforated with a thumb lancet, without difficulty or danger, but of that occlusion which results from a partial or total absence of the vagina. I can find but six cases reported since 1820, although I have carefully examined many of the leading Journals of England and the United States, for 30 years past. The operation, therefore, herein reported, has not been often performed, it is one of some interest, and for this reason I am induced to publish it.

Norfolk, Va., July 16, 1853.

THE MEDICAL EXAMINER.

PHILADELPHIA, AUGUST, 1853.

DEATH OF NATHANIEL CHAPMAN, M. D.

The declining health of Professor Chapman, for some years past, and his advanced age, had long since prepared us for the announcement of his death, which took place in this city on the 1st of July last. He had been, for the last three years, entirely withdrawn both from professional duties and general society, and his death was the result rather of a slow and gradual decay than of any positive disease.

Dr. Chapman was a native of Virginia, where he was born, in Fairfax Co., on the 28th of May, 1780. His family was of an old and respectable English stock; his paternal ancestor, who came to Virginia with the first colony, having been a captain of cavalry in the British army, and the youngest son of a cousin-german of Sir Walter Raleigh. The family settled on the river Pomonkey, some twenty miles from Richmond; but the branch from which the Doctor is descended, migrated, about a century and a half ago, to Maryland, and fixed itself on an estate on the banks of the Potomac, nearly opposite Mount Vernon, which is still, we believe, in their possession. The Doctor's father, however, went to Virginia upon his marriage, where he afterwards remained.

Dr. Chapman received his early education at the Classical Academy of Alexandria, founded by General Washington, where he passed six

years. He subsequently spent a short time in two colleges, though not long enough, as we have heard him say, to owe either any obligation. He came to Philadelphia in the autumn of 1797, and commenced the study of medicine with the late Professor Rush, of whom he became a a favorite pupil. He continued three years in the office of Dr. Rush, and in attendance upon lectures at the University of Pennsylvania, from which he received his degree in the spring of 1800. The Doctor's thesis was upon *hydrophobia*, written at the request of Dr. Rush, in answer to an attack upon his favorite theory of the pathology of that disease. Dr. Chapman had previously prepared another thesis, on the "Sympathetic connections of the stomach with the rest of the body," which he afterwards read before the Philadelphia Medical Society. This contained the substance of his peculiar views on fever and other diseases, as well as the modus operandi of medicines. While a student, Chapman found time to become a contributor to the Portfolio, a magazine of some celebrity in its day. His contributions, under the signature of Falkland, obtained considerable popularity.

In 1801, Dr. Chapman went abroad, where he spent four years—chiefly in Edinburgh and London. He remained a year in London, the private pupil of Abernethy, and thence passed to Edinburgh. Here he not only turned to account the advantages of the then celebrated school of medicine, but he enjoyed also the privilege of free intercourse with the distinguished literary and scientific circles of the modern Athens. He became intimate with many of the eminent persons of those days, among whom may be mentioned Dugald Stewart, the Earl of Buchan, and Mr. (afterwards Lord Chancellor) Brougham,* then a fellow student. Before Dr. Chapman's departure from Edinburgh, Lord Buchan gave him a public breakfast, on the birthday of Washington, at which a number of distinguished persons were present, when he took the occasion to entrust him with an interesting relic, valuable from a double historical association. Lord Buchan had presented to General Washington a box made of the oak that sheltered Sir William Wallace, after the battle of Falkirk, with a request to "pass it, in the event of his decease, to the man in his country who should appear to merit it best." General Washington, declining so invidious a designation, returned it by will to the Earl, who committed it to Dr. Chapman, to be delivered to Dr. Rush, with a view

* In 1809, Dr. Chapman republished, in Philadelphia, Mr. Brougham's speech in the House of Commons, on the British Orders in Council, with a biographical sketch of him, in which he predicted his future eminence. Lord Brougham was then quite a young man, little known in this country.

to its being ultimately placed in the cabinet of the College at Washington, to which General Washington had bequeathed a large sum.

Dr. Chapman returned to the United States in 1804. He established himself in Philadelphia, and in 1808, formed a matrimonial connection with Rebecca Biddle, daughter of Col. Clement Biddle, from which, during a period of nearly fifty years, he derived the highest degree of domestic happiness. His success in practice was rapid, and he became almost immediately the favorite physician of a large portion of the higher classes of Philadelphia, with whom he enjoyed a popularity as a practitioner, which has, perhaps, never been equalled here. A rare combination of personal qualities—attractive manners, wit, great conversational powers, and the kindest feelings—made Dr. Chapman as personally popular as he was professionally eminent.

As a writer and lecturer, Dr. Chapman long occupied a high rank. Soon after his return home, he published a work entitled "*Select Speeches, Forensic and Parliamentary,*" with critical and illustrative remarks, in five 8vo. volumes, which attracted much attention. He afterwards, however, confined his pen to scientific topics.

The year of his return to the United States, 1804, he gave a private course of lectures on Obstetrics, which proved so popular that, in 1806, at the age of twenty-six, he was elected adjunct to the Professor of Midwifery in the University of Pennsylvania, and soon afterwards he was elected to fill the chair of the Materia Medica. At the death of Rush, in 1812, he was transferred to the chair of Theory and Practice, which he filled from that date till his resignation in 1850. Professor Chapman's lectures, delivered as they were annually to large classes, for a period of nearly forty years, are of course familiar to no small portion of the profession of the United States; and it is almost superfluous in us to speak of them as erudite, elaborate and highly finished compositions, enriched with the stores of the most varied reading and the largest personal experience. They have been given to the public in various forms; many of them were published at length in the early volumes of this journal, and a compendium of them was subsequently issued, under the editorial supervision of Dr. N. D. Benedict, now of New York. Dr. Chapman's delivery of his lectures was animated, graceful, and emphatic. His voice had a slight nasal intonation, which was not unpleasant to the ear when familiarized with it. His lectures were read from the manuscript, but were frequently pointed and enlivened by the extemporaneous introduction of apt anecdote and humorous illustration.

In addition to his course at the University, Dr. Chapman for a long

period gave clinical lectures in the hospital of the Philadelphia Alms House. For nearly twenty-five years, also, he delivered a summer course of lectures in the Medical Institute of Philadelphia. This institution, which is still in existence, and is the oldest in the United States of the kind, was founded by Dr. Chapman, although he generously declined all participation in the fees, or control over the appointments to chairs.

Dr. Chapman has filled numerous honorable appointments in medical and learned societies. He frequently occupied the post of President of the Philadelphia Medical Society, and was the successor to Duponceau in the eminent distinction of the Presidency of the American Philosophical Society.

Dr. Chapman's principal work is his "*Therapeutics,*" published in 1817, which went through seven editions, one of them surreptitious. This treatise long maintained distinguished popularity among the works on Materia Medica, and now occupies a high rank as a book of reference. The author's views, however, on the modus operandi of medicines are of the ultra-solidist scoool, and, it is hardly necessary to add, not in accordance with received opinions.

In 1820 Dr. Chapman commenced the publication of the "Philadelphia Journal of the Medical and Physical Sciences," which he continued to edit for many years. The journal was undertaken with liberal views—the Doctor never receiving any salary for his services. He was subsequently an occasional contributor to different periodicals.

In the foregoing sketch we have presented a brief notice of the leading events in the professional career of Dr. Chapman. We leave, however, to other hands to do justice to the eminent social qualities which so long endeared him to all classes of the city in which the greater portion of his life was spent.

At the semi-annual meeting of the Faculty of Jefferson Medical College, held on the second day of July, 1853, the announcement of the decease of Professors HORNER and CHAPMAN having been made, it was resolved *unanimously*, that the Faculty, in common with their medical brethren, deeply deplore the loss to science of two individuals, the better part of whose valuable lives had been spent in the successful teaching of a profession of which they were distinguished ornaments, and to the advancement of which they had both so largely contributed.

Resolved, That a copy of this resolution be sent to the families of the deceased, and be published in the medical journals.

Extracted from the minutes. R. M. HUSTON, M. D.,
Dean of the Faculty.

MEDICAL NEWS.

CIRCULAR.

PHILADELPHIA, June 14, 1853.

To the Medical Profession.

In conformity with a resolution of the American Medical Association, it becomes my duty to inform you of the decision of the Committee of Publication, in regard to the price at which the forthcoming volume of Transactions will be furnished to the Association and others.

The price of *single copies* has been fixed by the Association at *five dollars.*

Any *individual* desiring two copies, will be furnished with them upon his remitting to the Treasurer *nine dollars.*

Societies, associations and institutions requiring *six* copies for the use of their members, will be supplied with them on remitting to the Treasurer *twenty-five dollars*, or they will be supplied with twenty-five copies on remitting *seventy-five dollars.*

Those who wish volumes 5 and 6, may obtain them by remitting *eight dollars.*

In consequence of the numerous illustrations, many of them highly finished colored lithographs, with which the forthcoming volume will be accompanied, its cost will considerably exceed that of either of those previously issued by the Association. To defray the cost of its publication, at least *fifteen hundred dollars*, in addition to the amount already received by the Treasurer, will be required.

Your attention is called to the following resolution, adopted at the last session of the Association:

"Resolved, That the Delegates from the several States be requested to appoint committees, who shall aid the Committee of Publication in procuring subscribers for and in distributing the annual Transactions of the Association."

D. FRANCIS CONDIE, *Treasurer,*
and Ch'm Com. Pub. A. M. A.

Dr. SOCRATES MAUPIN, late of the medical department of Hampden Sidney College, has been elected *Professor of Chemistry* in the University of Virginia, in the place of Dr. SMITH, resigned.

Dr. G. A. WILSON has been elected to the new chair of *Physiology and Medical Jurisprudence* in the medical department of Hampden Sidney College in Richmond, Va.

RECORD OF MEDICAL SCIENCE.

PATHOLOGY AND PRACTICE OF MEDICINE.

On employment of Ergot of Rye in some forms of Retention of Urine. By M. PASSOT, of Lyons.—Ergot of rye has not only the property of exciting the uterine contractions in cases of inactivity of the uterus, but is also very efficacious in the retention of urine which is caused by atony and paralysis of the bladder. MM. Baudin and Payan of Aix, were the first who endeavored to demonstrate that this agent does not act on the uterus alone, but rather on the lower part of the spinal cord. They also speak very highly of it as well in the affection which we have now under consideration, as in weakness or paralysis of the lower extremities.

Drs. Kinsley, Canuto-Canuti, Sainmont de Rocroy, Allier of Marcigny, have also recorded cases which bear favorable testimony to the utility of ergot in paralysis of the bladder. I will now briefly mention some of them:

Capt. B., aged 60, suffered from dysuria, which had increased greatly during the last three months, until it suddenly turned to a complete retention, which necessitated the employment of the catheter several times a day. For two months a host of remedies were used without avail; there was not the slightest improvement. The prostate became enlarged, and the patient suffered much from the use of the catheter, which had to be passed twice every day.

Fifty centigrammes (about eight grains) of ergot of rye, infused in a cupful of boiling water, was administered three times a day. At the expiration of six hours, the patient passed a small quantity of urine, and required the use of the catheter only once in the day. Afterwards it was only passed once in the forty-eight hours, and after ten days the bladder was left to itself (*Kinsley's Journal des Comm. Med. Chir.*, March, 1844.)

A lady, aged about 75, was affected with paralysis of the bladder, which for a time required the use of the catheter. Ergot was prescribed in doses of fifteen decigrammes (about twenty-eight grains) in infusion. On the sixth day of this treatment, it was no longer necessary to pass the instrument, the patient being able to pass water spontaneously (*Canuto-Canuti, Bull. des Sciences Med. de Bologne*, 1845.)

A man, named Rousseau, aged 58, of a nervous temperament, was, in consequence of a fit of passion, attacked with a complete inability to urinate. The bladder was obliged to be emptied by the catheter. The inertia of this organ continued in spite of cold injections to it, cold enemata, the application of ice, also a blister to the hypogastrium.

Strychnine applied, by means of ointment, as a dressing to the blister, also by frictions in the axillæ, on the following day produced cramps in the legs and arms, and was presently accompanied by stiffness, so that it became necessary to discontinue the use of this remedy. There was not the slightest action on the bladder. It was at this stage that the author

conceived the idea of giving ergot of rye. He prescribed six grammes (about ninety-two grains) coarsely powdered, to be put into a litre (about thirty-four ounces) of water, macerate for two days, filtered, and injected cold into the bladder. Seven minutes afterwards the patient experienced a desire to urinate, which, however, he could not then satisfy. The next morning the injection was again administered. Eight minutes after he had vesical tenesmus, and then spontaneous emission of urine. The injections were continued for some days. The cure was complete. (*Sainmont, Gazette des Hópitaux*, 1848.)

In 1848, Dr. Allier of Marcigny, sent a letter to the National Academy of Medicine, in which he gives as the result of his observations, that in only one out of fourteen cases ergot proved of no use.

I also am in possession of some cases which, in an incontestible manner, prove that ergot is capable of restoring the contractility of the bladder. The following is the most remarkable:—In the month of July, 1846, I was consulted by M. H., aged 60, of a dry constitution, and a very well-marked nervous temperament. Mr. H. admits having indulged in venereal excesses, and in the excesses of the table, and it is these that he blames for the vesical paralysis from which he is now auffering, and which requires the catheter twice a day: otherwise there is no symptom of organic alteration, no fever, no enlarged prostate. The canal of the urethra is free, through its entire length, and the urine when drawn off is perfectly clear. After having experienced the uselessness of tincture of cantharides and blistering the hypogastrium, I used the following prescription:—

Freshly powdered ergot . . .	2 grammes	(30 grains.)
Mucilage, ...	120 "	(31 ounces.)
A tablespoonful every half hour (shake the bottle.)		
Ergot of rye, powdered, . . .	15 decigrammes	(23 grains.)
Coca butter, ...	a sufficiency.	
To be made into two suppositories; one of them to be introduced night and morning.		

On the same day, at the expiration of some hours, M. H. felt a desire to micturate. At my evening visit I ordered a bath. The patient was scarcely in it before micturation took place spontaneously and with force. From this time to his death, M. H. has always passed water freely and without the assistance of the instrument. I should add, that to make certain of the cure, I continued the remedy for three or four days, but in a decreasing dose. M. H. died on the 30th January, 1848, of an acute pleuro-pneumonia, during the course of which not a single morbid symptom appeared in the bladder. It is therefore certain that ergot cures retention of urine which depends on pure and simple atony or paralysis of the bladder. But with regard to paralysis consecutive to apoplexy, or depending on other affections of the nervous centres, it is well known that they are unaffected by the remedy we are treating of.—*Med. Times and Gaz.*, from *Gaz. Med. de Lyon* and *Rev. Med. Chir. de Paris.*

Cysticercus in the Human Brain. By A. BRUNNICHE.—A servant girl, aged 43, of healthy frame and aspect, but for three years subject to epileptic fits, came to the hospital May 3, 1851, suffering from supposed ague. Upon admission, her symptoms were, localised pain all over the left part of the forehead and temples, weakness in the muscles of the face, sparks and flashes of light in the right eye, occasional dilatation of the pupil, squinting, marked diminution of the intellectual capacity, combined with erroneous ideas and hallucinations, slow, unconnected utterance, frequent vomiting, involuntary discharge of fæces, slow pulse, and weakness in the lower extremities. She had often complained of cold, and want of circulation in the fingers; so that, at the commencement, her illness had been mistaken for ague. She died May 21.

Examination of the Body.—Skull-cap thinner than natural; the surface of the brain bulged forwards from the cranial cavity; the membranes were thin, but full of blood. There were three cysticerci within the cranium. The largest lay in the substance of the anterior lobe of the left hemisphere in a cavity the size of a hen's egg, projecting into the anterior part of the lateral ventricle, and making a marked depression into the corresponding corpus striatum. One worm was contained in this cavity, but in no way connected with the wall. It was surrounded by a sac as large as a hazel-nut, whence proceeded other similar, but distinct prolongations. A second worm was found in the middle lobe of the left hemisphere, near the preceding; and a third in the posterior part of the right hemisphere, both invested in a surrounding membrane. The three worms were unconnected with one another, and in no organic union with surrounding parts; yet they had formed for themselves a well-organised capsule or bag. As regards the question, how these animals or their ova found their way into the human organism, the author believes that they must have been conveyed to the brain by the current of blood, after being taken up from the free surface of the body. Some authors have, in these cases, remarked no cerebral symptoms. In this there was marked disturbance of the intellectual faculties, although the substance of the brain was not in any way altered.—*Med. Times and Gaz.* from *Hospitals Meddelelser.*

SURGERY.

On the use of Collodion for Erections accompanying Blennorrhagia. By Dr. DORINGER.—In the *Med. Central Zeitung*, there is reported a case of a rather curious application of collodion for gonorrhœal erections, and the result was such as we would like to see borne out by other cases: A young man, aged 28, was attacked for the third time with a blennorrhagia, which was accompanied by such severe and painful erections, that the patient could hardly stay in bed half an hour. After having tried without avail both camphor and narcotics, Dr. Doringer ordered cold fomentations, and when the penis had resumed its natural size, the application over its whole extent, even including its prostatic portion, to a strong coating of collodion. This had the desired effect, for from that moment the patient had no erection, and suffered only from a slight

scalding in passing urine. What proves that the amelioration was really due to the means employed is, that on the morrow, the collodion being taken off, the erections returned, but not so severely, and again ceased on the application of a fresh coating of collodion.—*Med. Times and Gaz.*, from *Rev. Med. Chir. de Paris.*

Lithotomy.—The extraction of a urinary calculus from the bladder by the recto-urethral operation has been now successfully accomplished by Mr. Lloyd, in three instances, at St. Bartholomew's Hospital. The stone was removed quickly, and the patients have done well. It may, therefore, be interesting to state, that the proceeding resembles, in great measure, the operation performed by Vacca Berlinghieri, and described by Malgaigne. (*Manuel de Médicine Opératoire*, p. 686, 1843.) The subject having been placed as in the lateral operation, a staff is introdued into the bladder, and given to an assistant, who is directed to hold it in a vertical direction, so that the groove should correspond exactly with the mesial line. The index finger of the left hand is introduced into the rectum, the palmar surface being turned forwards. Upon its flat surface a straight bistoury is inserted to a depth of eighteen millimetres (about nine lines) from the margin of the anus. The handle of the knife being then depressed, the point is thrust through the walls of the intestine, and the edge being also carried upwards, the sphincter ani, the inferior part of the rectum, the perinæum from the anus to the bulb, and the cellular interval which separates these parts, are divided together. The inferior region of the prostate can then be felt through the wound. In front is felt the membranous portion of the urethra containing the staff. The nail of the index finger is next used, as in the lateral operation, to discover the groove in the staff, and to conduct to it the point of the bistoury. The staff is then withdrawn, and the bistoury is thrust into the bladder; the edge is turned downwards, so as to divide partially, but not completely, the prostate in the mesial line, care being taken to avoid re-entering the incision of the rectum. Therefore, this operation effects the division of those parts which were torn in the old operation with which the "apparatus major" was associated. Mr. Lloyd introduces a speculum into the rectum; his incision is, perhaps, somewhat deeper than that of Vacca Berlinghieri; he *dilates*, but does not cut the prostate, unless the stone should be of very considerable size.

The incision appears remarkaby small when the operation is concluded; and, as Malgaigne has observed, at each effort of excretion, the mucous membrane descends over the wound, which is thus placed under conditions most favorable to healing without leaving a fistula.—*Med. Times and Gaz.*

OBSTETRICS.

On Painful Distension of the Vagina after the Birth of a Child. By Dr. LEOPOLD.—Dr. Leopold states that he has several times met with examples (never in primiparæ) of excessive suffering, coming on from half an hour to an hour after the passage of the child, and referred to the vagina. It is of the most agonizing character, described by the women as worse than when the child is passing, and causing them to twist and

toss about in agony. It arises from distension of the vagina, either by accumulated coagula, or a very large placenta. In the first case, owing to the quantity of blood lost, there are always the early symptoms of uterine hæmorrhage; and the women, usually having suffered already from hæmorrhage in former labors, are dreadfully frightened at their danger. The softish but not distended uterus is felt pushed up into the umbilical region. The hand should be at once introduced into the vagina, and after the coagula are completely removed, it should be retained there for at least half an hour, keeping two fingers on the watch within the relaxed os uteri, and irritating this if required.

In the other case there is always a large placenta, which will not yield to moderate traction, partly because its size prevents it from easily traversing the vaginal passage, partly from a spasmodic action of the *constrictor cunni*, and partly because the membranes still remain in connection with the uterus to some extent. Notwithstanding the pain it will cause, the entire hand must be passed into the vagina, so as to embrace the whole placenta and bring it down.—*Neue Zeitschrift für Geburtskinde*, from *Brit. and For. Med.-Chi. Rev.*

MATERIA MEDICA AND THERAPEUTICS.

Cases of Poisoning with Lucifer Matches. By C. Trenerry, Esq., Surgeon of the Civil Hospital, Gibraltar, and Corresponding Member of the Surgical Society of Ireland.—Three cases, within a very short period, wherein Lucifer matches were swallowed for the purpose of self destruction, have presented themselves to my observation. Two of them proved fatal; the third recovered.

The history of two of these cases may be related in a few words: one, a young Spanish lady, resorted to the expedient from disappointment: the other was a Spanish widow, mother of three children, who, having committed herself with a young man, aged 18, hoped by taking the poisonous substance, to induce him to marry her. The contents of the stomach were dislodged, and she recovered.

The third case was that of a native of this garrison, aged 22, who was engaged to be married, but unfortunately too close an intimacy with her lover prior to this rash act exhibited itself, she being in the seventh month of pregnancy. Her aunt states that at no time were the symptoms very urgent, nor is it known when the matches were swallowed.

The deceased first complained of pain in the stomach at 5, A. M, on Tuesday, the 17th of May, 1853, for which some "Yerba Louisa" tea was given, which relieved her. At 8, A. M., she reluctantly partook of an egg and tea for breakfast, and at eleven o'clock had some broth, but could not bear the sight of bread. During the day she complained of thirst and giddiness: in the evening took tea and went to bed. The next morning, the 18th, about 6, A. M., still suffering from pain and giddiness, her aunt gave her a dose of castor oil, which she ejected immediately, and frequent vomiting supervened during the day. An enema of the decoction of marsh mallows and olive oil was administered, which afforded temporary relief. In the evening, she confessed that she had taken two boxes of Lucifer matches, containing about one hundred. A

medical man was immediately consulted, who ordered an emetic and olive oil, which she said relieved her very much; but vertigo, vomiting, intense thirst, slight convulsions, and syncope, followed in rapid succession, and after midnight she became insensible, and expired at 6, A. M. on the 19th. The following day, at noon, I made a post-mortem examination, and noted the following: Body well formed, but of short stature. Face has a distorted and dirty appearance from irregular patches of congestion. Globe of the eyes very prominent; cornea of a dry and dull appearance; pupils dilated, and blood oozing from the nostrils. Livid spots about the lips, gums and tongue. *Upper part of chest and shoulders quite green and emphysematous from decomposition.* Breasts plump, and nipples marked by darkish areola and papillæ. Abdomen distended and tympanitic, *but no appearance of decomposition*, except in the fold of the groins. Vagina plugged by a dark coagulum of blood, the os uteri dilated to about the size of a shilling, and the head of a child pressed against it, as if labor had commenced prior to death. Dark feculent, or tarry-looking matter escaping from the anus. Finger nails livid, as also back part of the anus and depending parts of the sides of the body, and in some parts it is quite green. Lower extremities pale and chloritic-looking; the calf of left leg being hard and globular, as if affected with cramp. Gas escaped with a hissing noise from the apertures made by the saw in opening the skull. Dura mater dry and glistening; substance of brain so much softened as to prevent the usual examination. Ventricles very dry. Cerebellum, a perfect pulpy or custard-like mass. The encephalon throughout of a dirty cream color. Pectoral muscles soft, and easily torn. Lungs dark-colored, but crepitant; their posterior portion congested, as is usually observed after death. Pericardium contained about two ounces of dark bloody serum. Heart pale and flabby; auricles and ventricles empty. Abdominal viscera had a dull appearance. Stomach greatly distended with gas; its external surface healthy-looking. Small intestines collapsed; large intestines not so distended as is usually seen. Anterior surface of liver pale. Gall-bladder empty. Spleen very dark-colored. Uterus contained a well-formed but dead female fœtus, of about seven or eight months, floating in the liquor amnii, the membranes being unbroken. The cellular tissue and depending or posterior portions of the liver, pancreas, spleen, and kidneys, unusually congested, amounting almost to extravasation of blood. The pharyngeal and cardiac end of the œsophagus of a dark or bruised color; the canal appears contracted and the mucous membrane dry, one rounded carbonacious head of a match adhering to its surface. Stomach contained about four ounces of a dark, unctuous treackly-looking fluid. The mucous membrane of a whitey-brown color, and the cardiac or larger end thickly vesicated, as if scalded; the middle portion retained its rugous appearance, although much thicker and paler than usual. The pylorous quite smooth, pale, and thickened, as also the mucous membrane of duodenum, which, with about three feet of the jejunum, contained a similar black-looking fluid as the stomach. The remaining portion of small intestines were merely lined with a creamy feculent substance interspersed with patches of greenish or bilious matter.—*Dublin Medical Press.*